

BOYLE

PHYSIOLOGICAL
ESSAYS

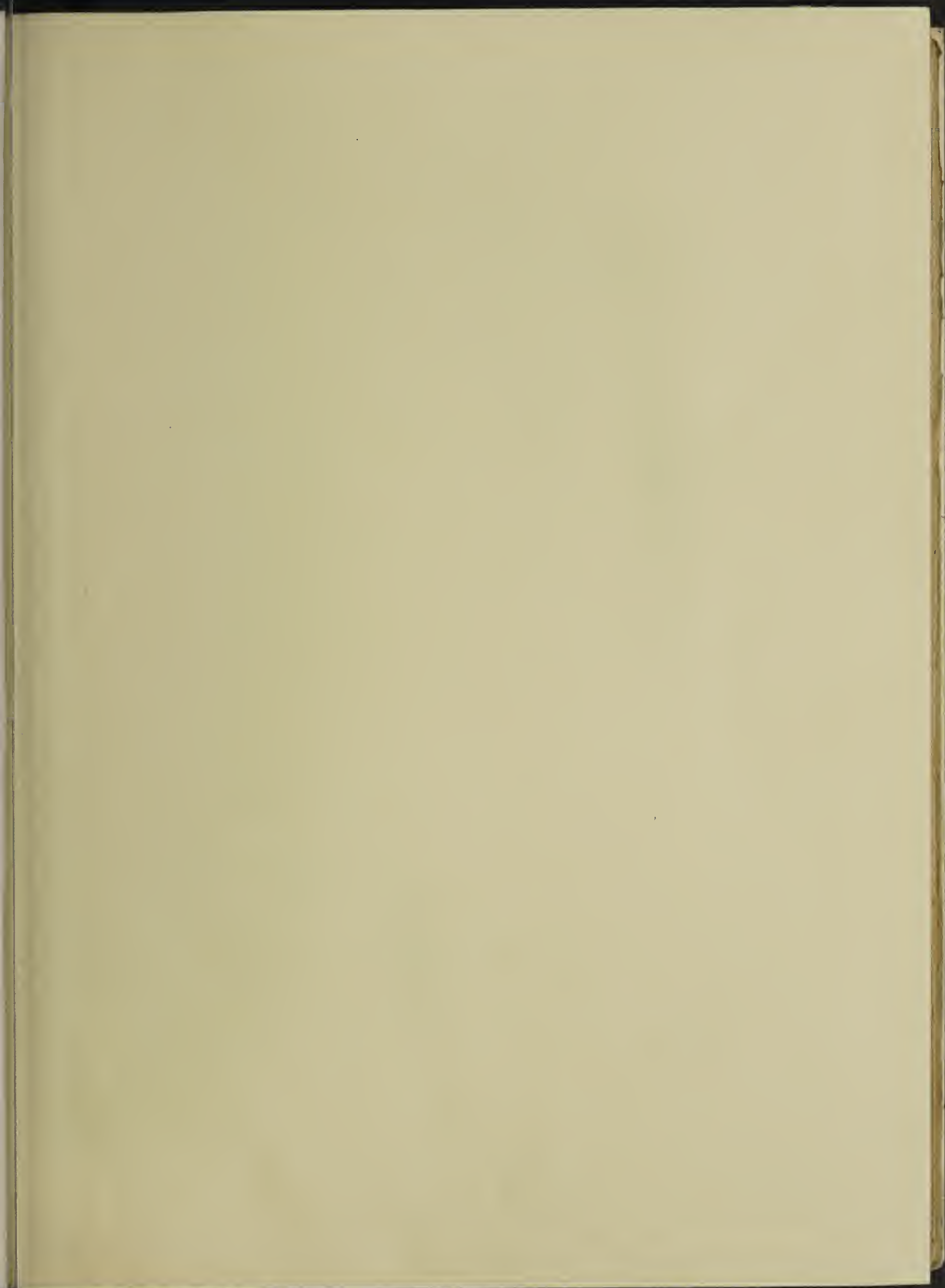
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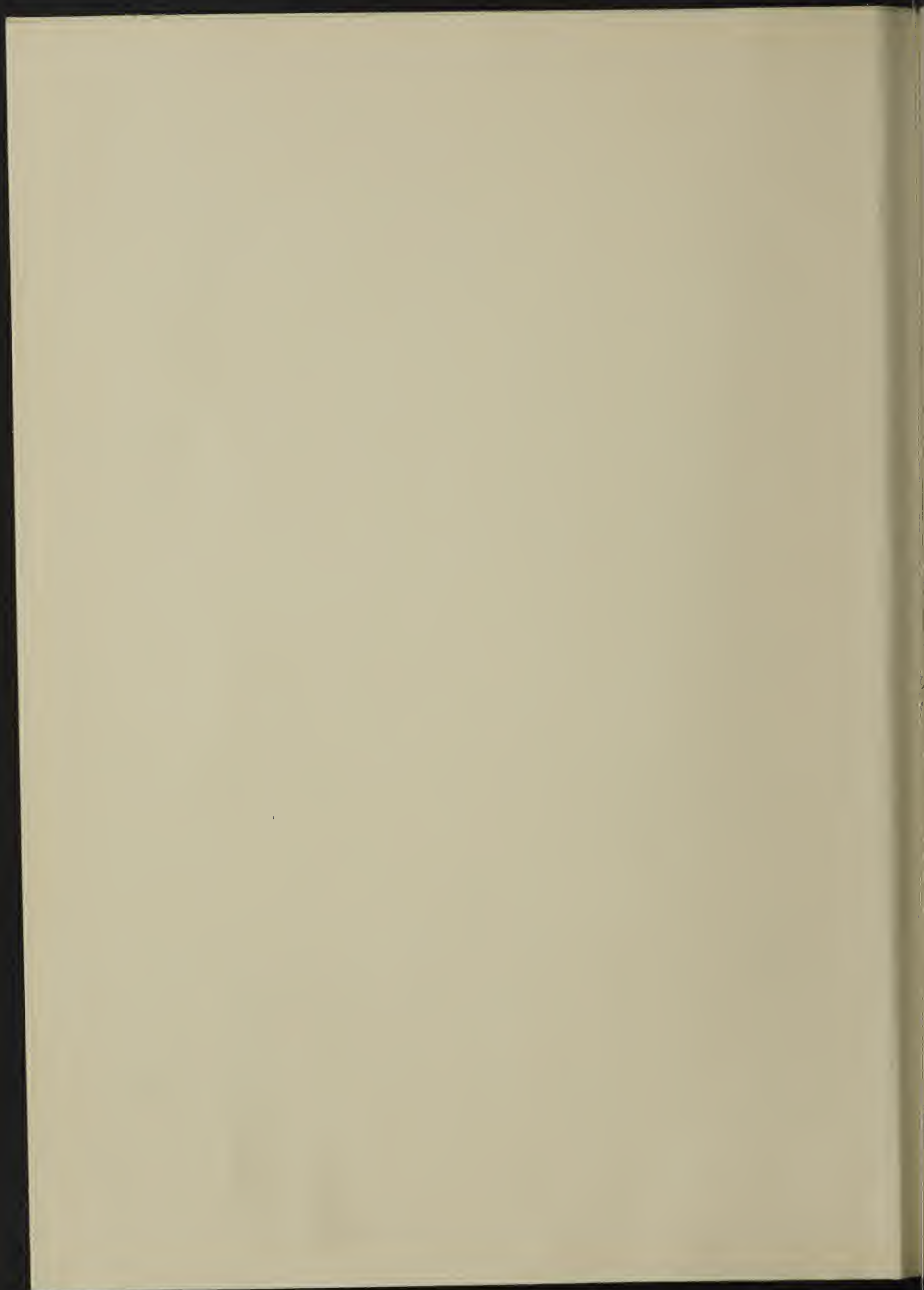


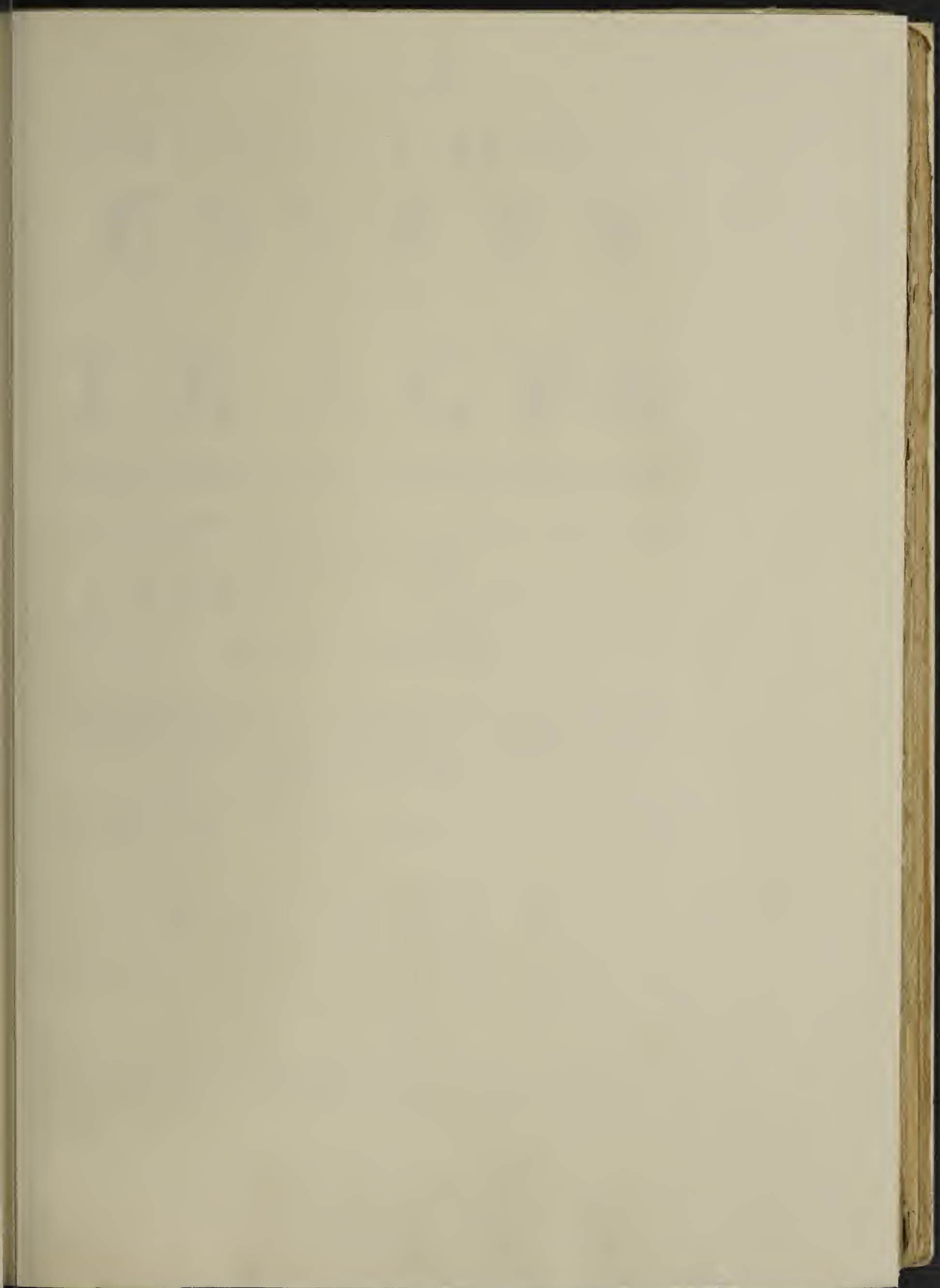


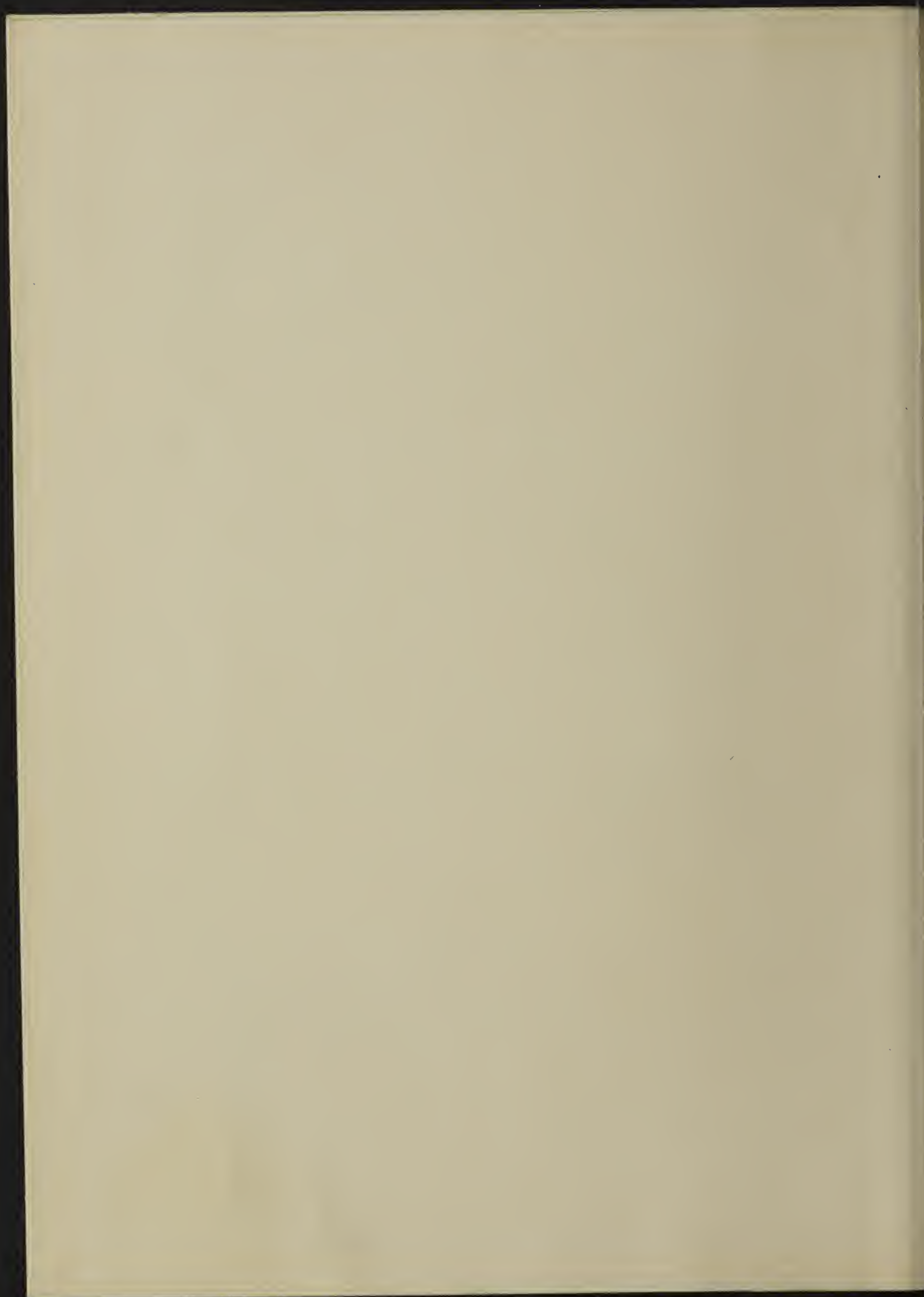


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CERTAIN
PHYSIOLOGICAL
ESSAYS

And other

TRACTS;

Written at distant Times, and on several Occasions.

By the Honourable
ROBERT BOYLE.

The Second Edition.

Wherein some of the Tracts are enlarged by Experiments,
and the Work is increased by the Addition of a
Discourse about the

ABSOLUTE REST
IN
BODIES.

LONDON,

Printed for *Henry Herringman* at the *Blew Anchor* in the
Lower Walk of the *New-Exchange*, MDC LXIX.

PHYSIOLOGICAL

By J. A. S. J.

The A. S. J.

1850



578

PHYSIOLOGICAL
ESSAYS.

LONDON,
Printed for *Henry Herringman*, MDCLXIX.

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REASAYS

Second to the First Edition
The second of 1850

THE SECOND OF 1850
BY W. D. D. D.



THE SECOND OF 1850
BY W. D. D. D.



AN
ADVERTISEMENT
TO THE
READER:

Prefixed to the First Edition,
Put forth A. D. 1661.



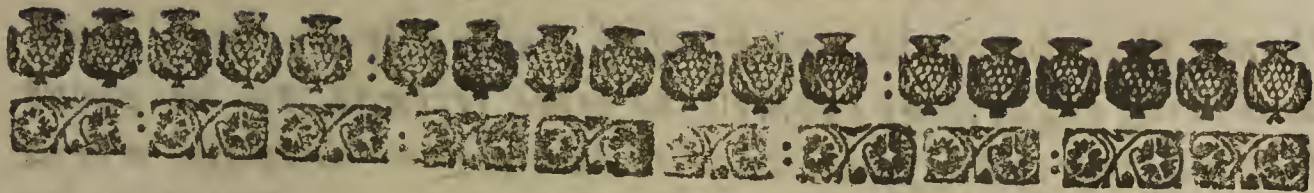
THE Publisher desires that the Reader should be inform'd, that neither were the following Treatises written near about the same Time (some of them being divers years elder than some others) nor yet are they now published in the same Order that they were written in.

For the first of these Discourses (though penned about four years since) was not only written after the second, third, and fourth, but after divers other Essays which the Author has yet lying by him among his Papers; it being intended for a kind of Introduction to all those Treatises which under several names, but chiefly that of Physiological Essays, the Author had then compos'd. But having during the late Confusions so disposed of his Papers to secure
A 3. them.

An Advertisement &c.

them, that he could not himself seasonably recover them; and being engaged by Promise to some friends, to let about half a dozen of his small Tracts come abroad into the world by such a time, he was fain to send the following Treatises to the Press as they came, some at one time, some at another, to his hands. And this his occasions did now and then reduce him to do in such haste, that he could not attend the correcting either the Printers Lapses or his own, and particularly was obliged, partly by haste, and partly by a Distemper in his Eyes, to send away the History of Firmness without so much as reading it over. All which 'tis hoped the Equitable Reader will consider in his favour, and bear with what may be imputable to such Circumstances.

I should add nothing further, were it not that to save the Reader the trouble of guessing who is meant by that Pyrophilus to whom most of the following Treatises are addressed, I think it requisite to inform him, that the person veil'd under that name, is that hopeful Young Gentleman Mr. Richard Jones, only Son to the Lord Viscount Ranelagh and an Excellent Lady, Sister to the Author.



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About the Second Edition.

FOr the ease of those that had already perused the first Edition of the Physiological Essays, and other Tracts that accompany'd them, the Author took the pains to include the principal things added in the present Edition in Paratheses, that the chief Additions may be found out without the pains of reading over again the whole Book. But finding these Paratheses to have been, by an oversight of the Press, omitted, some amends was thought fit to be endeavour'd to be made for that, by intimating that the Experiments that enlarge this New Edition, are, for the most part, to be found in the following Pages of it.

Page 196. 202. 222. 226. 227. 241. 243. 246. from pag. 252 to 255. p. 257. p. 238, 239. p. 263. from p. 289 to 291. p. 32 and 33. p. 169, 170.

Handwritten text at the top of the page, possibly a title or header, which is mostly illegible due to fading.

ADAMS & WESTBROOK

Main body of handwritten text, appearing to be a list or detailed notes, with some lines starting with capital letters.

Handwritten text at the bottom of the page, possibly a signature or a concluding note.

A

PROEMIAL ESSAY,

WHEREIN,

With some considerations touching

EXPERIMENTAL ESSAYS

in General,

Is interwoven such an Introduction to all those written by the Author, as is necessary to be perus'd for the better understanding of them.

I Know not, *Pyrophilus*, whether what you will meet with in the ensuing Discourses will prove worthy of your taking notice of it: Yet I dare tell you, that if all my Endeavours to serve you were not Duties, I should think I might deserve your Thanks for venturing to write them for your sake. For I am sufficiently sensible both how unlearned I am, and in how learned an Age I presume to write: Nor has the great number of those escap'd my Observation, who finding it a much easier task to censure Experimental Composures than to write such, endeavour to acquire the Title of Judicious, by condemning all things that themselves have not written, or thought on. And indeed, *Pyrophilus*, I had besides these, so many other discouraging Considerations in my Eye, whilst I was setting down the following Essays, that I should scarce have prosecuted a Design so full of trouble, and so unlikely by its success to make amends for

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it, if I had thought it free for the securing of my own Quiet and Credit, to suppress Observations which might prove serviceable to you, who having sufficiently convers'd with Books, are now desirous to begin to converse with Things themselves. But I must confess, I look upon Experimental truths as Matters of so great concernment to Mankind, that in spite of the just sense I have of my own Disabilities, I am deterr'd from complying with those Inclinations and Motives that endear silence to me, by considering the Fate of him, who though he had less entrusted to him than any of his Companions, was yet severely punisht for burying his single Talent. And though, *Pyrophilus*, I could not without such reluctancy resolve to write, yet I found it much more uneasy to resolve to write so soon: For I could not but consider, that being yet but very young, not only in Years, but, what is much worse, in Experience, I have yet much more need to learn, than ability to teach; and I consider'd too, that after a man is grown somewhat acquainted with things themselves, and has taken some general notice of the Cognations, Differences, and Tendencies of their Properties, he may every day so much improve his Knowledge, that I am apt to think, that if God should be pleas'd to protract my Life a few years longer, I shall at the end of them be able to look upon what I have hitherto written with Pity, if not with Blushes. And I have often observ'd, that it is wont to happen in the productions of the Mind, as in those of the Body. For as those that apply themselves to Procreation too young, and before they have attain'd to their full vigour and strength, do generally both hinder their own growth, and become the Parents but of weak and short-liv'd Children; so they that too early, and before their Judgment and Experience be fully ripe, addict themselves to write Books, do commonly both hinder their own Proficiency in Knowledge, and write but immature,

ture, and therefore seldom lasting Treatises. Nor should I, *Pyrophilus*, have ever prevail'd with my self to present you so early these Discourses, since by keeping them longer by me, I might easily by second Thoughts, and fresh Experiences be enabled to correct and enrich them, did not the frequent and dangerous distempers to which my very sickly Constitution has of late render'd me obnoxious, make me justly doubt, whether or no, if I should long forbear to write, Death would not sooner come than the expected Maturity of Age and Judgment. And though I had no such Consideration to move me to make hast to tender to you the ensuing Discourses, yet this would suffice to engage me to present them you with all their present defects; that if I should keep them till I can make them less unworthy of you, I must keep them till you are grown past the need of them.

And now that I have told you, *Pyrophilus*, both why I have written the ensuing Discourses, and why I keep them not by me long enough to present them you with fewer Imperfections, I suppose you will expect that I should next tell you why I have cast them into Essays, rather than into any other form. To satisfy you about this particular, *Pyrophilus*, I must freely acknowledge to you, that it has long seem'd to me none of the least impediments of the real advancement of true Natural Philosophy, that men have been so forward to write Systems of it, and have thought themselves oblig'd either to be altogether silent, or not to write less than an entire body of Physiology: for from hence seem to have ensu'd not a few Inconveniences.

And first, when men by having diligently study'd either Chymistry, Anatomy, Botanicks, or some other particular part of Physiology, or perhaps by having only read Authors on those Subjects, have thought themselves thereby qualify'd to publish compleat Systems of Natural Philo-

sophy, they have found themselves by the nature of their undertaking, and the Laws of Method, engag'd to write of several other things than those wherein they had made themselves Proficients, and thereby have been reduc'd, either idly to repeat what has been already, though perhaps but impertinently enough, written by others on the same Subjects, or else to say any thing on them rather than nothing, lest they should appear not to have said something to every part of the Theme which they had taken upon themselves to write of.

In the next place, the specious and promising Titles and comprehensive Method of these Systems have been often found to perswade unwary Readers, that all the parts of Natural Philosophy have been already sufficiently explicated, and that consequently it were needless for them to put themselves to trouble and charges in making further Enquiries into Nature, since others having already sufficiently made it their business to investigate and explicate Physiological Truths, our business needs now be no more than to learn what they have taught, and thankfully to acquiesce in it.

Nor has the Systematical way of writing been prejudicial only to the proficiency of some Readers, but also to the reputation of some Writers of Systematical Books. For it not unfrequently happens, that when a Writer, to vent some few peculiar Notions or Discoveries of his own, presumes to write a whole body of Philosophy, what is truly his own, though excellent in its kind, is either lost in the Crowd of the things he has borrow'd from others, and so comes to be over-look'd, or at least not sufficiently taken notice of, by the Reader; or else the unwelcome, and yet in such Composures scarce evitable, Repetition of many things that others had I know not how often written before, occasions the laying aside of the whole Book, as a Rhapsody of trite and vulgar Notions, scarce worth the perusing :

perusing : and by this means the Author often loses the Reputation of his peculiar Notions, as well as the Reader the benefit of them; and that which would have made an excellent and substantial Essay, passes but for a dull and empty Book.

But the worst Inconvenience of all is yet to be mention'd, and that is, That whilst this Vanity of thinking men oblig'd to write either Systems or Nothing, is in request, many excellent Notions or Experiments are by sober and modest men suppress'd, because such Persons being forbidden by their Judgment and Integrity to teach more than they understand, or assert more than they can prove, are likewise forbidden by Custome to publish their Thoughts and Observations, unless they were numerous enough to swell into a System. And indeed it may be doubted whether the Systematical Writers have not kept the world from much more useful Composures than they have presented it with. For there are very few men, if any at all, in the world, that are enrich'd with a competent stock of Experiments and Observations to make out clearly and solidly, I say not all the Phænomena of Nature, but all those that belong to Chymistry, Anatomy, or any such considerable subordinate Doctrine of Physiology. And those very men that are diligent and judicious enough to study prosperously any of those parts of Physiology, are oblig'd to spend so much time in the accurate Prosecution of that, and are wont to be thereby made so wary, and so thorowly acquainted with the difficulty of Physiological Investigations, that they will least of all men be forward to write Systems.

And what I say, *Pyrophilus*, of the inconveniences that have hitherto been observ'd to flow from mens forwardness to write entire bodies of Philosophy, may be in its degree appli'd to the practice of those that pretend to give us compleat accounts of Chymistry, or almost (I say almost)

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any other considerable and comprehensive part of Natural Philosophy: Though I deny not, that in such attempts which are much less difficult than the former, some Men have done Mankind considerable service, though they have not fully perform'd what the Titles of their Writings seem to promise. Nor am I so rigid as to be unwilling that from time to time some very knowing Writer should publish a System of Physiology, or any part of it, according to the best Authors and Observations of that time: For such a Work may be useful, partly, for the instructing of Youth in Schools and Academies; and partly, that men may have from time to time an Inventory of what hath been already discover'd, whereby the needless labour of seeking after known things may be prevented, and the progress of Mankind as to Knowledge might the better appear. But then it is to be wish'd that such Writings were not publish'd but by very intelligent Persons, nor till some considerable improvement have been made in Knowledge since the last work of that Nature. Nor would I be thought to disallow such Writings of very Learned Men, as though they may bear very general Titles, yet are not publish'd by their Authors as compleat Bodies or Systems of Physiology, but rather as general Principles (almost like the Hypotheses of Astronomers) to assist men to explicate the already-known Phænomena of Nature. For of such kind of Writings, if their Authors be (as for the most part they are) subtle and inquisitive men, there may be very good use, not so much by their gratifying the Intellect with the plausible account of some of Nature's Mysteries; as because on the one side their Writers, to make good their new Opinions, must either bring New Experiments and Observations, or else must consider those that are known already after a new Manner, and thereby make us take notice of something in them unheeded before; and on the other side, the curiosity of Readers, whether they
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like or disapprove the Hypothesis propos'd, is wont to be thereby excited to make trial of several things, which seeming to be Consequences of this new Doctrine, may by their proving agreeable or repugnant to Experiment either establish or overthrow it.

And that you may know, *Pyrophilus*, what kind of Writings I mean, I shall name to you the Learned *Gassendus* his little *Syntagma* of *Epicurus's* Philosophy, and that most ingenious Gentleman *Mons' Des-Cartes* his Principles of Philosophy. For though I purposely refrain'd, though not altogether from transiently consulting about a few Particulars, yet from seriously and orderly reading over those excellent (though disagreeing) Books, or so much as *Sir Francis Bacon's Novum Organum*, that I might not be prepossess'd with any Theory or Principles till I had spent some time in trying what Things themselves would incline me to think; yet beginning now to allow my self to read those excellent Books, I find by the little I have read in them already, that if I had read them before I began to write, I might have enrich'd the ensuing Essays with divers truths which they now want, and have explicated divers things much better then I fear I have done. But of such Writers the number is yet (and will I fear always be) so small, that I shall not need to make many Exceptions when I treat of the usefulness of writing Books of Essays, in comparison of that of writing Systematically: Or at least, *Pyrophilus*, whilst I presume not to judge of other mens abilities, I hope it may be lawful for me to confess freely to you concerning my self, that I am very sensible of my being far from having such a stock of Experiments and Observations, as I judge requisite to write Systematically; and I am apt to impute many of the Deficiencies to be met with in the Theories and Reasonings of such great Wits as *Aristotle*, *Campanella*, and some other celebrated Philosophers, chiefly to this very thing, that they have too
hastily,

hastily, and either upon a few Observations, or at least without a competent number of Experiments, presum'd to establish Principles, and deliver Axioms. For it very rarely otherwise happens, than that Theories that are grounded but upon few and obvious Experiments are subject to be contradicted by some such Instances as more free and diligent Enquiries into what of Nature is more abstruse, or even into the less obvious Qualities of things, are wont to bring to light. I remember, that being once at *Leyden*, I was brought to the Top of a Tower, where in a darken'd room (such as is now used in many places to bring in the Species of external Objects) a Convex glass, apply'd to the only hole by which light was permitted to enter, did project upon a large white sheet of Paper, held at a just distance from it, a lively representation of divers of the chief Buildings in the Town, all which upon the admission of more light into the room, by opening the Window, did immediately disappear. And methinks, *Pyrophilus*, that in divers of the Philosophical Theories that have been formerly applauded, something not unlike this may be easily observ'd: for though, whilst they are look'd on with such a weak and determinate degree of light, they may appear very artificial and well-proportion'd Fabricks, yet they appear so but in that twilight, as it were, which is requisite to their conspicuousness. For if but a full light of new Experiments and Observations be freely let in upon them, the Beauty of those (delightful, but Phantastical) structures does immediately vanish.

And truly, *Pyrophilus*, if men could be perswaded to mind more the Advancement of Natural Philosophy than that of their own Reputations, 'twere not methinks very uneasy to make them sensible, that one of the considerablest services that they could do Mankind, were to set themselves diligently and industriously to make Experiments and collect Observations, without being over-forward

ward to establish Principles and Axioms, believing it uneasie to erect such Theories as are capable to explicate all the Phænomena of Nature, before they have been able to take notice of the tenth part of those Phænomena that are to be explicated. Not that I at all disallow the use of Reasoning upon Experiments, or the endeavouring to discern as early as we can the Confederations, and Differences, and Tendencies of things : For such an absolute suspension of the exercise of Reasoning were exceeding troublesome, if not impossible. And as in that Rule of Arithmetick which is commonly called *Regula falsi*, by proceeding upon a conjecturally-supposed Number, as if it were that which we enquire after, we are wont to come to the knowledge of the true number sought for : so in Physiology it is sometimes conducive to the discovery of truth, to permit the Understanding to make an Hypothesis in order to the Explication of this or that difficulty, that by examining how far the Phænomena are, or are not, capable of being salv'd by that Hypothesis, the Understanding may even by its own Errors be instructed. For it has been truly observ'd by a great Philosopher, That Truth does more easily emerge out of Error than Confusion. That then that I wish for, as to Systems, is this, That men in the first place would forbear to establish any Theory, till they have consulted with (though not a fully competent number of Experiments, such as may afford them all the Phænomena to be explicated by that Theory, yet) a considerable number of Experiments in proportion to the comprehensiveness of the Theory to be erected on them. And in the next place, I would have such kind of superstructures look'd upon only as temporary ones, which though they may be preferr'd before any others, as being the least imperfect, or, if you please, the best in their kind that we yet have, yet are they not entirely to be acquiesced in, as absolutely perfect, or incapable of improving Alterations.

It were very possible, *Pyrophilus*, to let you see that all that has been said to recommend to you that form of Writing which (in imitation of the French) we call *Essayes*, is but a part of what may be pertinently said to the same purpose. But because this Introductory Discourse it self is to be but an *Essay*, not a Book, I dare not long insist upon the Advantages of this sort of Discourses. Only because I think that if I could engage you, *Pyrophilus*, and such other ingenious Persons, to cast their Physiological Observations and Reflexions into Experimental *Essayes*, I should thereby do real Learning no trifling service, by bringing so useful a way of writing into the request it deserves; Upon this consideration, I say, I must beg leave to represent to you this great Conveniency of *Essayes*, That as in them the Reader needs not be clogg'd with tedious Repetitions of what others have said already, so the Writer, having for the most part the Liberty to leave off when he pleases, is not oblig'd to take upon him to teach others what himself does not understand, nor to write of any thing but of what he thinks he can write well. And if such *Essayes* be but as they should be competently stock'd with Experiments, 'tis the Readers own fault if he be not a Learner by them: for indeed when a Writer acquaints me only with his own Thoughts or Conjectures, without enriching his discourses with any real Experiment or Observation, if he be mistaken in his Ratiocination, I am in some danger of erring with him, and at least am like to lose my time, without receiving any valuable Compensation for that great loss: but if a Writer endeavours, by delivering new and real Observations or Experiments, to credit his Opinions, the Case is much otherwise; for let his Opinions be never so false, his Experiments being true, I am not oblig'd to believe the former, and am left at liberty to benefit my self by the later; and though we have erroneously superstructed upon his Experiments, yet the
foun-

foundation being solid, a more wary builder may be very much further'd by it in the erection of more judicious and consistent Fabricks : such a Writer, if I be not wanting to my self, will certainly teach me useful Truths, and if it be not my fault, he can lead me into no errors; and oftentimes the very Experiments that he delivers, besides that they may be applicable to many other purposes unthought of by him, may be either sufficient or at least helpful to the very discovery of the erroneousness of the Opinions they are alledg'd to countenance : and I make account that a man that gives me, together with his conjectures (though erroneous) in matters of Physiology, some noble Experiment or Observation by which he pretends to verify them, does me no greater injury than *Galileo* upon his first Invention of the Telescope would have done an Astronomer, if he had told him, that he had discover'd in Heaven those imaginary new Stars which a late Mathematician has fancy'd himself to have descry'd there, and at the same time had made him a Present of an excellent Telescope, with expectation that thereby the Receiver should be made of the Giver's Opinion ; for by the help of his Instrument the Astronomer might not only make divers useful Observations in the Sky, and perhaps detect new Lights there, but discern also his mistake that gave it him.

After what has been said, *Pyrophilus*, of the Usefulness of experimental Essays, we must proceed to say something concerning the Manner of writing them : but because you will also expect to receive some account of the ensuing Discourses, I shall not treat of those two Subjects apart, but, in discoursing of the following Essays, shall take occasion to acquaint you with part of my thoughts concerning such kind of Compositions in general, the other Considerations belonging to the same Subject being already upon several Occasions dispers'd among, and to be met with in, the ensuing Discourses themselves.

And first, as for the style of our experimental Essays, I suppose you will readily find that I have endeavour'd to write rather in a Philosophical than a Rhetorical strain, as desiring that my expressions should be rather clear and significant, than curiously adorn'd : For, to a subject of the serious and important Nature of Physiology, that saying may unquestionably be appli'd, *Ornari res ipsa negat, contenta doceri.* And certainly in these Discourses, where our design is only to inform Readers, not to delight or perswade them, Perspicuity ought to be esteem'd at least one of the best Qualifications of a style, and to affect needless Rhetorical Ornaments in setting down an Experiment, or explicating something abstruse in Nature, were little less improper than it were (for him that designs not to look directly upon the Sun it self) to paint the Eyeglasses of a Telescope, whose clearness is their Commendation, and in which even the most delightful Colours cannot so much please the eye as they would hinder the sight. And that it may not be suspected, that those that would not have it requisite to imploy a florid style in treating of Philosophical Subjects, do but in their own excuse deny the necessity of such Rhetorical Embellishments as they are not able to afford their Composures, give me leave to subjoyn, that it was not an unpolish'd Naturalist, but that Prince of Orators, *Cicero* himself, who made this studious Declaration, *Omne (says he) quod de re bona dilucidè dicitur, præclarè mihi dici videtur : istiusmodi autem res velle ornatè dicere, puerile est ; planè autem & perspicuè expedire posse, docti & intelligentis Viri.* But I must not suffer my self to slip unawares into the Common place of the unfitness of too spruce a style for serious and weighty matters ; and yet I approve not that dull and insipid way of writing which is practis'd by many Chymists, even when they digress from Physiological Subjects : for though a Philosopher need not be sollicitous that his style

style should delight its Reader with his Floridnesse, yet I think he may very well be allow'd to take a Care that it disgust not his Reader by its Flatness, especially when he does not so much deliver Experiments or explicate them, as make Reflections or Discourses on them; for on such Occasions he may be allow'd the liberty of recreating his Reader and himself, and manifesting that he declin'd the Ornaments of Language, not out of Necessity, but Discretion, which forbids them to be us'd where they may darken as well as adorn the Subject they are appli'd to. Thus (to resume our former Comparison) though it were foolish to colour or enamel upon the glasses of Telescopes, yet to gild or otherwise embellish the Tubes of them, may render them more acceptable to the Users, without at all lessening the Clearness of the Object to be look'd at through them.

And as for Exotick Words and Terms borrowed from other Languages, though I expect that Persons not conversant in the Philosophical Composures written (especially of late) in our Language will be apt to suspect me for the willing Author of divers new Words and Expressions, yet as for you, *Pyrophilus*, who peruse other then Moral; Theological, and Historical Books in English, and find how much use is made in them of Exotick Terms, I hope you will find that I have not at all affected them, but have rather studiously declin'd the use of those which Custom has not render'd familiar, unlesse it be to avoid the frequent and unwelcome Repetition of the same word, (so troublesome to the Ear, and so much forbidden by Orators) or for some peculiar significancy of some such Word, whose Energy cannot be well express'd in our Language, at least without a tedious Circumlocution. And in such cases, *Pyrophilus*, I suppose a Writer may be allow'd to use Exotick Terms, especially when Custom has not only Denizon'd them, but brought them into request. For as in the Fashions of Clothes, though perhaps Fools begin them

them, yet Wise men, when they are once generolly receiv'd, scruple not to follow them, because then obstinately to decline them would be as ridiculously singular as at first it would have been to begin them: so in Exotick Words, when Custom has once made them familiar and esteem'd, scrupulously to decline the use of them may be as well a fault, as needlessly to imploy them: For it is not the Use but the Affectation of them that is unworthy a Philosopher. And from the latter of those I hope I have kept my self far enough: For I should think my self guilty of a very Childish vanity, if the use I made of Languages were so to write as to be the less understood. But besides the unintentional deficiencies of my style, I have knowingly and purposely transgress'd the Laws of Oratory in one particular, namely, in making sometimes my Periods or Parentheses over-long: for when I could not within the Compass of a regular Period comprise what I thought requisite to be delivered at once, I chose rather to neglect the Precepts of Rhetoricians, than the mention of those things which I thought pertinent to my Subject, & useful to you, my Reader. And for this fault, *Pyrophilus*, since I have made my self guilty of it but for your sake, I think I ought to obtain your pardon at least as easily as my own, since barely to keep you from losing any thing that I conceiv'd might be serviceable to you, I knowingly expose my style to be censur'd as disproportionate to it self.

The next thing, *Pyrophilus*, of which I am to give you an account, is, why I have in the ensuing Essays deliver'd many Experiments and Observations, which may seem slight and easie, and some of them obvious also, or else perhaps mention'd by others already. To satisfy you about this, I must inform you that many of the Particulars which we are now considering, were in my first Design collected in order to a Continuation of the Lord *Verulam's Sylva Sylvarum*, or Natural History. And that my intended
Centuries

Centuries might resemble his, to which they were to be annex'd, it was exquisite that such kind of Experiments and Observations as we have been newly speaking of, should make up a considerable part of them. And indeed it were to be wish'd, that such inquisitive Persons as cannot be at the Charge, or have not the opportunity, of making new Experiments, would busie themselves, as they have opportunity, in industriously collecting and carefully setting down the Phænomena to be met with without the Assistance of new Experiments, especially such particulars as seem either to be of moment in order to the hinting or Confirmation of some Considerable Truth, or to the Detection of some Applauded Error, or else to have bin, though obvious enough, yet little taken notice of. For I am confident that very much may be done towards the Improvement of Physiology by a due Consideration of and reflexion on the obvious Phænomena of Nature, and those things which are almost in every bodies power to know, if he please but seriously to heed them; and I make account that attention alone might quickly furnish us with one half of the History of Nature; as well as industry is requisite, by new Experiments, to enrich us with the other. And therefore I confess I think my self beholding to him that first makes me take notice of what I might easily have known, but heeded not before; it not seldom happening, that we are prejudic'd by, though we do not complain of that ignorance from which we might relieve our selves, if we did but diligently turn our eyes to the Observations wherewith even neighbouring and familiar Objects would, if duly consulted, present us. But I digress, and therefore must step back into the way, and tell you, that the reasons why I first design'd the Narrative of what I had try'd and observ'd for a Continuation of *Sir Francis Bacons* Natural History, you will meet with in my Preface to that specimen of the intended Continuation, which I have given in those
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Of my Essays that treat Of Promiscuous Experiments : and the reason why I have since declin'd that succinct way of Writing, is, for the sake of *Pyrophilus*, that I might have, in a more free and uncircumscribed way of discoursing, a greater Liberty to insist on and manifest the Reasonableness of such Animadversions as I thought seasonable for a Person, who, though a great Proficient in the other parts of Philosophy, is but a Beginner in Experimental Learning. And the second Reason why I have often made use of seemingly slight Experiments, is, because such are more easily and cheaply try'd, and they being alledg'd for the most part to prove some Assertion, or credit some Admonition, I thought their Easiness or Obviousness fitter to recommend them, than depretiate them ; and I judg'd it somewhat unkind, or at least indiscreet, to refer you most commonly for proof of what I deliver'd, to such tedious, such difficult, or such intricate Processes, as either You can scarce well make, unless You be already what I desire my Experiments should help to make You, a skilful Chymist ; or else are as difficult to be well judg'd, as the truth they should discover is to be discern'd. I was also hopeful that the Easiness of divers things inviting you to make tryal of them, and keeping You from being disappointed in Your Expectations, the success of Your first attempts would encourage You to make tryal also of more nice and difficult Experiments. And till You have try'd them, do me the right to think that I deal not unsincerely with You.

The Reasons of my having divers times recorded Experiments which You may have formerly met with, and perchance ev'n in Printed Books, I have elsewhere deduc'd in a peculiar Discourse on that Subject : and therefore shall now only add, that by reason of my being as yet a stranger to the German Tongue, wherein the most and best Chymical Books are said to be written, I may have set down
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divers Chymical Experiments and Observations that are extant already in that Hermetical Language, (if I may so call it) without having had them from their Dutch Publishers, or so much as dream'd of their having been divulg'd by any man. I have likewise in my Preface to the Essays that you will meet with under the Title of Promiscuous Experiments, given You an account why I have not refrain'd from mentioning divers things which may seem very slight, because very obvious: And I have long had thoughts to inform You in an intire Discourse to be written on purpose, why I think that ev'n the trivial, and therefore slighted, truths of Physiology ought not to be despis'd. And for my own part, I shall not scruple to confess to You, that I disdain not to take Notice ev'n of Ludicrous Experiments, and think that the Plays of Boys may sometimes deserve to be the Study of Philosophers: For as when we go a Hunting, though the flight of the Hare and the pursute of the Dogs be to us but sport and recreation, yet the Beasts themselves are extreamly earnest, the one to save his threatn'd life by flight, and the other to overtake his desired Prey; so Nature acts very seriously in all the other things that we make sports with, and is in very good earnest, whether we Men be so or no.

Perhaps you will wonder, *Pyrophilus*, that in almost every one of the following Essays I should speak so doubtfully, and use so often, *Perhaps, It seems, 'Tis not improbable*, and such other expressions as argue a diffidence of the truth of the Opinions I incline to, and that I should be so shy of laying down Principles, and sometimes of so much as venturing at Explications. But I must freely confess to you, *Pyrophilus*, that having met with many things of which I could give my self no one probable cause, and some things of which several Causes may be assign'd so differing, as not to agree in any thing unless in their being all of

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them probable enough, I have often found such Difficulties in searching into the Causes and Manner of things: and I am so sensible of my own Disability to surmount those Difficulties, that I dare speak confidently and positively of very few things, except of Matters of fact. And when I venture to deliver any thing by way of Opinion, I should, if it were not for meer shame, speak yet more diffidently than I have been wont to do. 'Tis not that I at all condemn the Practice of those Inquisitive Wits that take upon them to explicate to us ev'n the abstrusest Phænomena of Nature: For I am so far from censuring them, that I admire them when their Endeavours succeed, and applaud them ev'n where they do but fairly attempt. But I think 'tis fit for a man to know his own Abilities and Weaknesses, and not to think himself oblig'd to imitate all that he thinks fit to praise. I know also that the way to get Reputation, is, to venture to explicate things, and promote Opinions: For by that course a Writer shall be sure to be applauded by one sort of men, and be mention'd by many others; whereas by the way of Writing to which I have condemn'd my self, I can hope for little better among the more daring and less considerate sort of men, should you shew them these Papers, than to pass for a Drudge of greater Industry than Reason, and fit for little more than to collect Experiments for more rational and Philosophical heads to explicate and make use of. But I am content, provided Experimental Learning be really promoted, to contribute ev'n in the least plausible Way to the Advancement of it, and had rather not only be an Underbuilder, but ev'n dig in the Quarries for Materials towards so useful a Structure, as a solid body of Natural Philosophy, than not do something towards the Erection of it. Nor have my thoughts been altogether idle and wanting to themselves, in framing Notions, and attempting to devise Hypotheses, which might avoid the deficiencies.

cies observ'd in other mens Theories and Explications : but I have hitherto, though not always, yet not unfrequently, found that what pleas'd me for a while, as fairly comporting with the Observations on which such Notions were grounded, was soon after disgrac'd by some further or new Experiment, which at the time of the framing of those Notions was unknown to me, or not consulted with. And indeed I have the less envy'd Many (for I say not All) of those Writers who have taken upon them to deliver the Causes of things, and explicate the Mysteries of Nature, since I have had opportunity to observe how many of their Doctrines, after having been for a while applauded and even admir'd, have afterwards been confuted by the discovery of some new Phænomenon in Nature, which was either unknown to such Writers, or not sufficiently consider'd by them. For I have found it happen as well to many others (that have publisht their Opinions) as to me (who have been more private in my Guesses) in our Theories built on either too obvious or too few Experiments, what is wont to happen to the Falsifiers of Coyn : for as Counterfeit pieces of Money will endure some of them One Proof, as the Touch-stone, others Another, as Aqua fortis, some a third, as the Hammer or the Scales, but none of them will endure All proofs ; so the Notions I mention (in which sort I fear too great a part of those hitherto extant may be compriz'd) may agree very fairly with this or that or the other Experiment, but being made too hastily, and without Consulting a competent number of them, 'tis to be fear'd that there may still after a while be found one or other, (if not many) their Inconsistency with which will betray and discredit them.

I have notwithstanding all this on some occasions adventur'd to deliver my Opinion, not that I am very confident of being less subject to erre in those particulars than in any of the others wherein I have refrain'd from interposing any

Conjecture, but because I would manifest to You, that I scruple not to run the same venture with those incomparably better Naturalists, that have thought it no disgrace in difficult matters rather to hazard the being sometimes mistaken, than not to afford Inquisitive Persons their best Assistance towards the Discovery of Truth.

And because, *Pyrophilus*, in the Reasons and Explications I offer of Natural Effects, I have not for the most part an immediate recourse to the Magnitude, Figure, and Motion of Atoms, or of the least Particles of Bodies, I hold it not unfit to give You here some account of this Practice, not so much for the sake of those few Passages in my Essays that may be concern'd in it, as for that of many Learned men, especially Physitians, whose useful Writings begin to be undervalu'd, and are in danger to be despis'd, by an Opinion taken up from the mis-understood Doctrine of some eminent Atomists, as if no speculations in Natural Philosophy could be rational, wherein any other causes of things are assign'd than Atoms and their Properties. I consider then, that generally speaking, to render a reason of an Effect or Phænomenon, is to deduce it from something else in Nature more known than it self, and that consequently there may be divers kinds of Degrees of Explication of the same thing. For although such Explications be the most satisfactory to the Understanding, wherein 'tis shewn how the effect is produc'd by the more primitive and Catholick Affection of Matter, namely, bulk, shape and motion, yet are not these Explications to be despis'd, wherein particular effects are deduc'd from the more obvious and familiar Qualities or states of Bodies, such as Heat, Cold, Weight, Fluidity, Hardness, Fermentation, &c. Though these themselves do probably depend upon those three universal ones formerly nam'd. For in the search after Natural Causes, every new measure of Discovery does both instruct and gratifie the Understanding, though

I readily confess, that the nearer the discover'd Causes are to those that are highest in the scale or series of Causes, the more is the Intellect both gratify'd and instructed.

I think it therefore very fit and highly useful, that some speculative Wits well vers'd in Mathematical Principles and Mechanical Contrivances, should employ themselves in deducing the chiefest Modes or Qualities of Matter, such as are Heat, Cold, &c. and the States or conditions of it, (if we think fit to distinguish these from its Qualities) as fluid, firm, brittle, flexible, and the like, from the above-mention'd most primitive and simple Affections thereof. And I think the Common-wealth of Learning exceedingly beholden to those Heroick Wits that do so much as plausibly perform something in this kind. But I think too, we are not to despise all those Accounts of particular Effects which are not immediately deduc'd from those primitive Affections of either Atoms or the insensible Particles of Matter, but from the familiar, though not so universal, Qualities of things, as cold, heat, weight, hardness, and the like. And perhaps it would be none of the least advantages which would accrue to Naturalists from a satisfactory explication of such Qualities by the most primitive and simple ones, that it would much shorten the explication of particular Phænomena: For though there be many things in Nature that may be readily enough made out by the Size, Motion, & Figure of the small Parts of Matter, yet there are many more that cannot be well explain'd without a great deal of Discourse, and divers successive Deductions of one thing from another, if the purpos'd effect must be deduc'd from such primary and Universal Causes; whereas if we be allow'd to take the Notions of Cold, Heat, and the like Qualities for granted, the explications and proofs may be much more compendiously made. He gives some Reason why Stones and Iron and all other heavy Bodies will swim in Quick-silver, except
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Gold, which will sink in it, that teaches, that all those other Bodies are *in specie* (as they speak) or bulk for bulk, lighter than Quick-silver, whereas Gold is heavier. He, I say, may be allow'd to have render'd a Reason of the thing propos'd, that thus refers the Phænomenon to that known Affection of almost all Bodies here below, which we call Gravity, though he do not deduce the Phænomenon from Atoms, nor give us the cause of Gravity, as indeed scarce any Philosopher has yet given us a satisfactory Account of it. So if it be demanded, why, if the sides of a blown Bladder be somewhat squeez'd betwixt ones hands, they will, upon the removal of that which compress'd them, fly out again, and restore the Bladder to its former figure and dimensions, it is not saying nothing to the purpose, to say that this happens from the spring of those Aerial Particles wherewith the Bladder is fill'd, though he that says this be not perhaps able to declare whence proceeds the Motion of Restitution, either in a Particle of compress'd Air, or any other bent spring.

And as for the Reasons of things assign'd by Physitians, they must be most of them despis'd, unless we will allow of such explications as deduce not things from Atoms or their Affections, but only either from secondary Qualities, or from the more particular Properties of Mixt Bodies. If a Physitian be ask'd why Rhubarb does commonly cure Loosenesses, he will probably tell you as a Reason, that Rhubarb is available in such Diseases, because it hath both a Laxative vertue, whereby it evacuates Choler, and such other bad humours as are wont in such cases to be the peccant Matter, and an astringent Quality, whereby it afterwards arrests the Flux: But if you further ask him the Reason why Rhubarb purges, and why it purges Choler more than any other humour, 'tis ten to one he will not be able to give you a satisfactory answer. And indeed, not only the manner whereby Purgative Medicines Work,
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but those other Properties whereby some Bodies are Diuretick, others Sudorifick, others Sarcotick, &c. are not I fear so easie to be intelligibly made out as men imagine, and yet a skilful Physitian would justly think himself wrong'd, if the Reasons he renders of things in his own Profession were deny'd the Name of Reasons, because made without recourse to Atomical Principles. And indeed, there are oftentimes so many subordinate Causes between particular Effects and the most General Causes of things, that there is left a large field wherein to exercise Mens Industry and Reason, if they will but solidly enough deduce the Properties of things from more general and familiar Qualities, and also intermediate Causes (if I may so call them) from one another. And I am the more backward to despise such kind of Reasons, because I elsewhere declare, that there are Some (for I do not say, Many) things, as particularly the Origine of Local Motion, of which ev'n by the Atomical Doctrine no Physical Cause can well be render'd; since either such things must be ascrib'd to God, who is indeed the true, but the supernatural Cause of them, or else it must be said, (as it was by the old Epicureans) that they did ever belong to Matter, which, considering that the Notion of Matter may be compleat without them, is not to give a Physical efficient cause of the things in Question, but in effect to confess that they have no such Causes. But of this elsewhere more.

In the mean time, that you may not be drawn away to undervalue such Writers as I have been pleading for, nor think you ought to refrain from writing what occurs to you, though true and useful, unless you deduce it, or at least can do so, from the Epicurean Notions, I shall here briefly represent to you, (what perhaps you will not hereafter think a despicable suggestion) that there are two very distinct Ends that Men may propound to themselves in studying Natural Philosophy. For some Men care only
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to Know Nature, others desire to Command Her : or to exprefs it otherwise, some there are who desire but to Please themselves by the Discovery of the Causes of the known Phænomena, and others would be able to produce new ones, and bring Nature to be serviceable to their particular Ends, whether of Health, or Riches, or sensual Delight. Now as I shall not deny but that the Atomical, the Cartesian, or some such Principles, are likely to afford the most of satisfaction to those speculative Wits that aim but at the knowledge of Causes ; so I think that the other sort of men may very delightfully & successfully prosecute their ends, by collecting and making Variety of Experiments and Observations, since thereby learning the Qualities and Properties of those particular Bodies they desire to make use of, and observing the power that divers Chymical Operations, and other ways of handling Matter, have of altering such Bodies, and varying their effects upon one another, they may by the help of Attention and Industry be able to do many Things, some of them very Strange, and more of them very Useful in humane life. When a Gunner or a Souldier employs Gun-powder, it is not necessary that he should consider, or so much as know, of what and of how many Ingredients (much less of what kind of Atoms) it is made, and the proportion and manner wherein they are mingled ; but the Notice Experience gives him of the power of that admirable Concrete, as it is made up and brought to his hands, suffices to enable him to perform things with it, that nothing but their being common and unheeded can keep from being admir'd. The Physitian that has observ'd the Medicinal vertues of Treacle, without knowing so much of the names, much less the Nature of each of the sixty and odd Ingredients whereof it is compounded, may cure many Patients with it. And though it must not be deny'd, that it is an advantage as well as a satisfaction, to know in general how the Qualities
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of things are deducible from the primitive Affections of the smallest parts of Matter, yet whether we know that or no, if we know the Qualities of this or that Body they compose, and how 'tis dispos'd to work upon other Bodies, or be brought on by them, we may, without ascending to the Top in the series of Causes, perform things of great Moment, and such as without the diligent Examination of particular Bodies would, I fear, never have been found out *à priori* ev'n by the most profound Contemplators. We see that the Artificers that never dream'd of the Epicurean Philosophy, have accommodated Mankind with a Multitude of useful Inventions. And *Paracelsus*, who (besides that he seems none of the most piercing and speculative Wits) sure had little recourse to Atomical Notions, if he ever so much as heard of them, was able to perform some things that were truly admirable, besides those he vainly boasted of; as may appear not only by what I elsewhere represent, but by what *Oporinus* himself (as severely as he otherwise writes against his deserted Master) confesses he saw of the stupendous cures which *Paracelsus* wrought with his famous *Laudanum*, (whatever he made it of.) But we need not go far to find a noble Example to our present purpose, since we see that the bare making of tryals with the Load-stone, and Irons touch'd by it, though the Experimentors were ignorant (as some fear we yet are) of the true and first Causes of Magnetical Phenomena, have produc'd Inventions of greater use to Mankind, than were ever made by *Leucippus*, or *Epicurus*, or *Aristotle*, or *Telesius*, or *Campanella*, or perhaps any of the speculative Devisers of new Hypotheses, whose Contemplations aiming for the most part but at the solving, not the encreasing or applying, of the Phenomena of Nature, it is no wonder they have been more ingenious than fruitful, and have Hitherto more delighted than otherwise benefitted Mankind: I say Hitherto, because though Expe-

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rience warrants me so to speak now, yet I am not unwilling to think that Hereafter, and perhaps in no long time, when Physiological Theories shall be better establish'd, and built upon a more competent number of Particulars, the Deductions that may be made from them may free them from all Imputation of Barrenness. But of these things I elsewhere (though not as I remember in any of the following Essays) more fully discourse.

And therefore I shall now resume the Subject that occasion'd this long Excursion, and add to what I said in excuse of my venturing sometimes to deliver something as my Opinion in difficult or controverted cases, that I must declare to you, *Pyrophilus*, that as I desire not my Opinions should have more Weight with you than the Proofs brought to countenance them will give them, so you must not expect that I should think my self oblig'd to adhere to them any longer than those Considerations that first made me embrace them shall seem of greater Moment than any that I can meet with in opposition to them. For *Aristotle* spoke like a Philosopher, when to justify his Dissent from his Master *Plato*, he said among other things, That for the sake of Truth, men (especially being Philosophers) ought to overthrow ev'n their own Tenents (*Δόξει δ' ἂν ἴσως βέλτιον εἶναι, καὶ δεῖν ἐπὶ σωτηρίᾳ γε τῆς ἀληθείας καὶ τὰ ὀκνεῖα ἀναρῆναι, ἄλλωστε καὶ φιλοσόφους ὄντας.*) And though for a man to change his opinions, without seeing more reason to forsake them than he had to assent to them, be a Censurable Levity and Inconstancy of mind; yet to adhere to whatever he once took for truth, though by Accession of more light he discover it to be erroneous, is but a proud Obstinacy very injurious to Truth, and very ill becoming the sense we ought to have of humane frailties. And it ought to be esteem'd much less disgraceful to quit an Error for a Truth, than to be guilty of the Vanity and Perverseness of believing a thing still, because we once believ'd it. And certainly,

Ethic.
Nicom.
lib. 1.
cap. 6.

certainly, till a Man is sure he is infallible, it is not fit for him to be unalterable. X

You will easily discern, *Pyrophilus*, that I have purpose-ly in the ensuing Essays refrain'd from swelling my Discourses with solemn and elaborate Confutations of other mens Opinions, unless it be in some very few Cases, where I judg'd that they might prove great impediments to the Advancement of Experimental Learning; and even such Opinions I have been wary of meddling with, unless I suppos'd I could bring Experimental Objections against them. For 'tis none of my Design to engage my self with or against any one Sect of Naturalists, but barely to invite you to embrace or refuse Opinions as they are consonant to Experiments, or clear Reasons deduced thence, or at least analogousthereunto, without thinking it yet seasonable to contend very earnestly for those other Opinions which seem not yet determinable by such Experiments or Reasons. And indeed, to allude to our former Comparison, I would endeavour to destroy those curious but groundless structures that men have built up of Opinions alone, by the same way (and with as little Noise) by which such fantastical structures as those I mention'd to have seen at *Leyden* may be demolish'd. To destroy which 'twere needless to bring battering Engines, since nothing is requisite to that effect but an encrease of Light. And Experience has shown us, that divers very plausible and radicated Opinions, such as that of the Unhabitableness of the Torrid Zone, of the Solidity of the Celestial part of the World, of the Blood's being convey'd from the Heart by the Veins (not the Arteries) to the outward parts of the body, are generally grown out of request, upon the appearing of those new Discoveries with which they are inconsistent, and would have been abandon'd by the Generality of Judicious Persons, though no man had made it his business purposely to write Confutations of them: so true is that

Vulgar saying, that *Rectum est Index sui & Obliqui.*

But when at any time, *Pyrophilus*, I have been induc'd to oppose others, as I have not deny'd my self the freedom that is requisite unto Loyalty to Truth, so I have endeavour'd to use that Moderation and Civility that is due to the persons of deserving Men. And therefore you shall find me not only in one Essay oppose an Author whom in another I applaude, but sometimes you may find me commending a Writer in the very same Page, perhaps, where I am endeavouring to disprove his Opinions: For I love to speak of Persons with Civility, though of Things with Freedom. Nor do I think it reasonable, either that any mans Reputation should protect his Errors, or that the Truth should fare the worse for his sake that delivers it. And as for the (very much too common) Practice of many, who write, as if they thought, railing at a mans Person, or wrangling about his Words, necessary to the Confutation of his Opinions; besides that I think such a quarrelsome and injurious way of writing does very much misbecome both a Philosopher and a Christian, methinks it is as unwise, as it is provoking. For if I civilly endeavour to reason a man out of his Opinions, I make my self but one work to do, namely, to convince his Understanding: but if in a bitter or exasperating way I oppose his Errors, I encrease the Difficulties I would surmount, and have as well his Affections against me as his Judgment: and it is very uneasie to make a Profelyte of him that is not only a Dissenter from us, but an Enemy to us. And that which makes me the more dislike the bitter way of disputing which I am reprehending, is, that I have often observ'd, that though one of the Disputants alone be at first in the fault, yet the other is most commonly drawn to share in the Guilt, though to contract it he must imitate his Adversary. For as a mad Dog by biting others is wont to make those he bites run mad like himself, and do promiscuous
Mischief;

Mischief; so these so provoking Writers are wont to enrage those they offend, and infect them also with their own virulent distemper. But, *Pyrophilus*, when I speak of dealing respectfully with those I dissent from, I would be understood of such as have well deserved of Experimental Learning, or at least been candid and sober Enquirers after Truth. For, as I think that it would much discourage any prudent Person from venturing to communicate what he thinks he knows to the World, to find that an Error proceeding from humane Weakness, or the Darkness and Abstrusity of things, seldom escapes being detected without being made matter of disgrace or reproach to the Author: so on the other side, when vain Writers, to get themselves a name, have presum'd to obtrude upon the credulous World such things, under the Notion of Experimental Truths, or even great Mysteries; as neither themselves ever took the pains to make tryal of, nor receiv'd from any credible Persons that profess'd themselves to have try'd them; in such cases, I see not how we are oblig'd to treat Writers that took no pains to keep themselves from mistaking or deceiving, nay, that car'd not how they abuse us to win themselves a name, with the same respect that we owe to those, who though they have mis'd of the Truth, believ'd they had found it, and both intended to deliver It, and took some (though not prosperous) pains that they might convey nothing else to us.

I fear it will be requisite, *Pyrophilus*, to tell you why in some of the following Essays you will meet with many Passages transcrib'd out of other Authors, and in some very few or none at all. And therefore to give you first a short Account of the Particular mention'd last, I must mind you, that 'twas most suitable both to my Humor and Design to deliver only those things wherewith my own Observations, or Tryals, or Thoughts, had furnish'd me, without troubling you with the Repetition of those things which
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had been deliver'd by others already ; those kind of Repetitions, unless they be made upon some such grounds as we shall presently mention, seeming to me to be as vainly as ambitiously affected by many Writers, and being deservedly troublesome to Judicious Readers, who can easily discern that they add much more to the bulk of Books than of Knowledge. But this notwithstanding, *Pyrophilus*, I thought my self oblig'd on some occasions, for your sake, to overcome my Natural Averseness to stuff any Writings of mine with Passages transcrib'd from those of others, partly for the Reasons elsewhere insisted on, and partly for divers others. As First, because some Particulars are of that Strangeness, and of that Moment, that they need and deserve to be verify'd by more than a single Attestation. Next, because according to the Greek Proverb, *νοινὰ καινῶς*, it is not properly to say over the same thing again, when the Observation, Experiment, or other Passage of an Author, is either illustrated or otherwise improv'd by the Repetition, or else apply'd to some purpose differing from that to which the Author brought it : That being applicable to many a single Experiment or Observation which *Seneca* somewhere says, *Nulla Res consummata est dum incipit* ; And, *Etiam si omnia à Veteribus inventa sunt, hoc semper novum erit, Usus, & Inventorum ab aliis Scientia & Dispositio*. And thirdly, because as the Planets and other Stars have (according to Astrologers) in their great Synods or Conjunctions, much more powerful, and sometimes other Influences on the Air and some other sublunary Parts of the World, than are ascrib'd to one or two of them out of that Aspect ; so divers Particulars, which whilst they lay single and scatter'd among the Writings of several Authors were inconsiderable, when they come to be laid together in order to the same Design, may oftentimes prove highly useful to Physiology in their Conjunction, wherein one of them may serve to prove one part

Questi-
on. Na-
tural.
lib. 6.
cap. 5.
Epistola
64.

or circumstance of an important Truth, and another to explicate another, and so all of them may conspire together to verifie that Saying, *Et quæ non prosunt singula, multa juvant.* It may then I hope suffice to justifie me on this occasion, that not appealing to other Writers as to Judges, but as to Witnesses, nor employing what I have found already publish'd by them barely as Ornaments to imbellish my Writings, and much less as Oracles by their Authority to demonstrate my Opinions, but as Certificates to attest Matters of fact, I may hope that their Testimonies will as well be illustrated by mine, as mine by their's, and that all of them may contribute to your better Information.

And if, *Pyrophilus*, you grant that upon these Considerations I have not done amiss to apply to my purpose divers of those things which I found deliver'd pertinently to them by those Writers which I chanc'd to cast mine Eyes on, I suppose you will not think I need to make you an Apology for my having made most use of the Passages of those Writers which I suppose will be most difficult to be met with (such as are many Books of Navigations and other Voyages) and especially of French Books not yet translated into English or Latin. And I think I shall less need to make an Excuse for my having for the most part set down the Passages I recited in the Authors own Words, that being one of the readiest ways I know to satisfy the Reader, and avoid injuring the Writer. And indeed, I have met with abundance of Quotations wherein the Transcriber doth so mistake, or so mis-represent the cited Authors Meaning, sometimes out of Inadvertence, but sometimes too I fear out of Indulgence to his own Hypothesis, that if ever I should be tempted to trouble the World with any of my thoughts, I would beseech my Readers, not to look upon any thing as my Opinion or Assertion that is not deliver'd in the entire Series of my own Words;

Words ; lest a Transcriber should make me deliver those things resolutely and dogmatically, which I deliver but hæsitantly and conjecturally ; and lest I should seem to set down those things Positively as Processes for whose success I undertake, which I record but by way of Narrative.

For my so frequently mentioning what I have borrow'd from other Writers, or receiv'd from my friends, I expect to be excus'd by that of *Pliny*, *Benignum est (ut arbitror) & plenum ingenui Pudoris, confiteri per quos profeceris.* Though I have seen divers Modern Writers that so boldly usurp the Observations and Experiments of others, that I might justly apply to them what the same *Pliny* annexes ; *Scito enim, conferentem Authores me deprehendisse à juratissimis & proximis Veteres transcriptos ad Verbum, neque nominatos, &c.* If other Writers should not prove more equitable (for I will not say more thankful) than such as these, they would quickly discourage those whose aims are not very noble and sincere, from gratifying the Publick with Inventions, whose Praise and Thanks would be usurp'd by such as will not name them. But perhaps they would be more just if they reflected on what our Author further adds, *Obnoxii profectò animi & infelicitis ingenii est, deprehendi in furto malle quam mutuam reddere, cum præsertim sors fiet ex Usura.*

And now I have said this concerning the Passages I have borrow'd from other Authors, it will not be improper to add something about those I have declin'd to borrow. For you may possibly marvel, that in divers of the Historical parts of my Writings I have omitted such Testimonies either of *Pliny*, *Solinus*, *Aristotle*, *Theophrastus*, *Ælian*, or perchance some of the ancient Physitians themselves (who yet, as more conversant with things, are usually more credible) as seems very pertinent to my Discourse, and fit to prove what I design. But when I shall come to entertain
you

you about Natural History, I doubt not but to satisfy you with the Reasons I shall offer you of this Practice. In the mean time, I shall only tell you in short, that though I have a just respect for those Great Names I have mention'd ; yet the sense I have of the difficulties, I have found to make and relate an Observation accurately and faithfully enough for a Naturalist to rely on ; and the occasions I have had of looking into divers matters of fact deliver'd in their Writings, with a bold and an impartial Curiosity ; have made me conclude so many of those Traditions to be either certainly false, or not certainly true, that except what they deliver upon their own particular Knowledge, or with peculiar Circumstances that may recommend them to my belief, I am very shy of building any thing of moment upon foundations that I esteem so unsure, and much less upon the suspected Passages that *Wecker*, *Paracelsus*, *Porta*, &c. abound with. And therefore (though I well enough know how much I impoverish my Discourse by this Niceness) yet I do not think it fair to imploy that as an Argument to convince you that has not that operation upon me myself. And I the rather take notice of my forbearing to make use of the Historical Traditions and Chymical or Magical Secrets that I meet in the above-mention'd Authors, or any other makers of Collections, unless the Narrative be (as I was saying) expressly enough deliver'd upon the Writers Personal Knowledge, or that of some other credible Witness ; not only because I would give you an account why several of my Writings are unfurnish'd with what most Readers look on as the richest Ornaments of other mens, but because if this wariness could be introduc'd, t'would be the most effectual way of perswading men to write those kind of Tracts I would recommend, Physiological Essays. For he that will confine himself so strictly, will scarce be often tempted on

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Physical

Physical Subjects, to write either Systems or Volumns.

Another thing, *Pyrophilus*, I must needs advertise you of in reference to the ensuing Discourses, which is, That besides those Deficiencies in point of Ratiocination which are due to my personal Disabilities, I have purposely let pass some Few (and but very Few) Inferences which I discern'd well enough not to be cogent, because I was willing to acquaint you upon some particular Occasions with all the Experiments then occurring to me, which I thought might contribute to the Illustration of the Subject in hand, though each of them apart did not irrefragably, nor indeed so much as strongly infer the Conclusion in order to which they seem'd to have been mention'd as Premisses. And this Practice I made the less scruple of, because I was willing to exercise thereby your Reasoning Faculty, and try how far you would discern the Tendency of several things, all of them pertinent enough to the Subject in hand, but not all of them concluding to the main design in order whereunto they were alledg'd. And I supposed that the things by me mention'd, though not conclusive, being yet Experimental, the mention of them, which in a strictly Logical way of reasoning must have been forborn, might well make you amends for the Exercise to which I intended they should put your Reason.

There remains yet one thing, *Pyrophilus*, of which I suppose you will expect I should give you an Account ; and that is, why in the ensuing Essays I have mention'd divers Experiments which I have not plainly and circumstantial-ly enough delivered. To satisfy you concerning which, I must represent to you, First, That though for your sake I have oftentimes, contrary to my Reason and Genius, deliver'd things, to make them more clear, in such a Multitude of words, that I now seem even to my self to have in divers places been guilty of Verbosity ; yet in some
other

other passages, treating of things which Use had render'd very familiar to me, I may have, to shun Prolixity, unawares slipt into the Contrary Extream. Secondly, There are some Mechanical Experiments wherein I have purposefully omitted some manual Circumstances, because I was unwilling to prejudice some ingenious Trades-men, who make either a Livelyhood, or at least a gain, by the sale of the productions of such Experiments. And I made the less scruple to conceal such Mechanical Circumstances, (if I may so call them) because they were not necessary to the Physiological Knowledge of the Experiments: in naming of which my intention was to teach you rather Philosophy than Trades. Thirdly, I mention'd some things but darkly, either because I receiv'd them upon Condition of secrecy, or because some ingenious persons that communicated them to me, or others to whom I imparted them, do yet make, and need to make, a pecuniary advantage of them. Fourthly, And some things that, either having been the fruits of my own Labours, or obtain'd in Exchange of such, are freely at my own disposal, I have not yet thought fit so plainly to reveal, not out of an envious design of having them bury'd with me, but that I may be always provided with some Rarity to barter with those Secretists that will not part with one Secret but in Exchange for another, and think nothing worth their desiring that is known already to above one or two Persons. And I think it very lawful to reserve always some conceal'd Experiments by me, wherewith to obtain the secrets of others, which being thereby gained, the other (as being no longer necessary to the former end) may freely be communicated.

And think not, *Pyrophilus*, that the bare mention of an Experiment as having been performed, though the way of making it be conceal'd, is of no use, if the Relator of the

Experiment be a Person that may safely be credited : For it is something to be assur'd that such and such things have been really perform'd, and consequently are possible to be done, though we be not yet particularly acquainted with the means of performing them. And he tells you something, that tells you upon his own Knowledge, that in such or such Bodies, or Ways of operating on them, considerable things of such or such a Nature are to be met with. And for my part, when I go a Hawking or Setting, I think my self beholden to him that assures me that in such a field there is a Covey of Partridges, though he does no more towards the giving me them. And it is obvious how much *Europe* is beholden to *Columbus* for the Detection of many Countries in *America* which were not discover'd by him, nor perhaps till long after his Death, because he first inform'd us Knowingly that there were unknown Regions beyond that vast Ocean which severs the Old World from the New. But I begin to digress, and therefore shall proceed to tell you, That I am the less troubled at my Omission of the circumstantial parts of some Experiments, because I think it will be much for your Advantage to try them over again your self. And as I have taken care by the truth of the Experiments I have deliver'd to secure your success, in case you try them aright ; so I cannot be very sorry that you should in some Particulars have a kind of Necessity laid on you to exercise your own industry, and thereby encrease your Experience.

But besides all that has been said, *Pyrophilus*, I must freely confess to you, that there is one thing particularly relating to your self, which has made me refrain from delivering in the ensuing Essays some of the chief Chymical processes wherewith they might have been enrich'd. For not yet knowing with what seriousness you will addict
your

your self to promote experimental Philosophy, nor what use you will make of what has been unveiledly communicated to you, I was somewhat unwilling that some things which had cost me a great deal of pains should yet fall into any man's hands that scorns to purchase Knowledge with some pains, and I was desirous, in case you shall prove as industrious as I hope you will, to have something by me to encourage and cherish your industry, which may be more suitable to your improved Knowledge. For I must confess to you, that in reference to the Chymical processes extant in the following Discourses, I look upon most of them but as trifles, not only in comparison of those things which a knowing Chymist might have deliver'd on the same subjects, but even in regard of divers processes (not impertinent to those discourses) wherewith I my self, (as little as I am a Pretender in these Matters) am not unacquainted: and perhaps I would have given to the following Treatises the Title of Trifles, instead of that of Essays, if I had not been afraid of discouraging you thereby, and if the Chymical part of them had been the chief thing wherewith I intended to acquaint you in them. But if the reception you give to what we have already written, prove such as may encourage us to proceed, we may perhaps, if God be pleas'd to vouchsafe us Life and Opportunity, be invited to impart to you those more considerable Chymical Experiments, which either the Communication of our friends or our own Labours have presented us. For it will be much in the power of the Entertainment which these Papers shall meet with, to make them either the Beginning of our Labours of this nature, or the End. And in the meantime, I think I may venture to tell you, that, as inconsiderable as I have confess'd divers of the Chymical Processes mention'd in these Essays to be, yet if ever you take the pains (as I hope you will) to write
 Experi-

Experimental Essays, and confine your self to take as little upon trust as I have done, you will perhaps be ready to believe, that sometimes a short Essay of this nature, not to say some one single Experiment, may have cost me more than a whole Treatise written on such a Subject, whereon to be able without Discredit to write Books, it is almost sufficient to have read many. And give me leave to add, that as in such kind of Composures, oftentimes the enabling himself to give a considerable Advertisement, or even Hint, may cost the Writer more than the making of several Experiments; so it may be also more beneficial to the Reader than the Knowledge of them. For we must not always measure the Considerableness of things by their most obvious and immediate usefulness, but by their fitness to make or contribute to the discovery of things highly useful. As, if it be true, what is reported by good Authors of the hazel Wand, or *Virgula divinatoria*, though the hazel Tree be much less considerable in reference to its fruit, or immediate productions, than a Peach-Tree, an Orange-Tree, or even an Apple-Tree; yet may it be made much more valuable than any of them, because whereas they only present us with fruits, this may assist us to discover in latent Mines inestimable Treasures.

I had almost forgot to advertise you, *Pyrophilus*, That whereas I have not been so sollicitous as most Writers are wont to be, to swell the ensuing Essays with the Enumeration of the various Opinions and Arguments of Authors about the Subjects I treat of, or to adorn them with acute Sentences, fine Expressions, or other Embellishments borrow'd from eminent Writers; it has not been, because I utterly dislike the making use of those passages in Classick or other Authors, that may either give (among the Admirers of those Writers) some Authority to our thoughts, or very handsomely and Emphatically express them.

them. For I remember, I have known it reprehended by Learned Men in *Epicurus*, That though he writ very much himself, he would not vouchsafe in his Writings to quote those of other Men. And that I have not refrain'd from making use, now and then, of those Philological Ornaments of Discourse, when they readily occur'd to me, and appear'd neither impertinent nor prolix, may I hope suffice to keep me from being suspected of the Vanity of thinking my self above other mens assistance. But the reasons of my so much declining to make use of other mens Authority, or Expressions, were chiefly these. First, That the Weakness of my Eyes has this long time kept me from reading almost any Books, save the Scripture, with some Critical Expositions of it, and here and there some Portions of the Writings of those that pretend to teach their Readers Experimental Matters: And the unfaithfulness of my Memory as to things of no great Moment, has made me forget almost all the little Philological and florid Learning I was formerly acquainted with. And really, *Pyrophilus*, as for the Books that treat of Natural Philosophy, I am so sensible of the smalness of the Advantage which my Disabilities have suffer'd me to make of them, that instead of being ambitious to appear a great Reader, I could be very well content to be thought to have scarce look'd upon any other Book than that of Nature. And in the next place, *Pyrophilus*, though I ignore not that by this plain and unadorn'd way of Writing, I unkindly deny my Essays many Embellishments which I might give them, and which perhaps you will think they do abundantly need; yet my frequent Distempers, Journeys, and other Avocations, not allowing me so much time as I desir'd, to entertain you on Philosophical subjects, I thought it more requisite to spend those confin'd hours in acquainting you with my own thoughts, such as they are, than with those of
other

other Men ; unless (as I formerly intimated) I can some way or other more than barely recite what I recite of theirs. And you will easily pardon me the injury which for your sake I do my own Reputation by this naked way of writing, if you, as well as I, think those the profitablest Writers, or at least the kindest to their Perusers, who take not so much Care to appear Knowing Men themselves, as to make their Readers such.

TWO
ESSAYS,

Concerning the Unsuccessfulness

OF

EXPERIMENTS,

CONTAINING

Divers Admonitions and Observations (chiefly
Chymical) touching that SUBJECT.



LONDON,

Printed for Henry Herringman, Anno 1668.

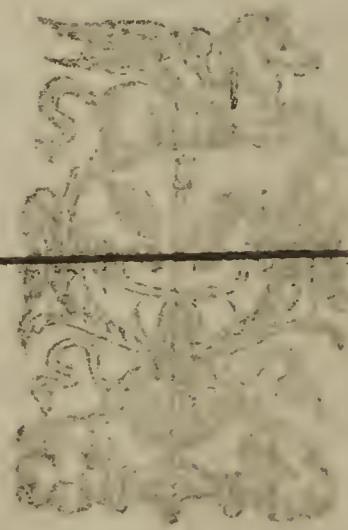
TWO
ESSAYS

Concerning the Principles

of
EXPERIMENT

Advertisement about the two following Essays.

THe Author of these Discourses had enlarged them in this second Edition with divers Observations and Experiments, but that he has made use of them already in other Papers belonging to his Sceptical or Doubting Naturalist.



LONDON
Printed for J. B. Roper, at the Golden Ball, in St. Dunns Church-yard, 1704.



THE FIRST
E S S A Y.

Of the Unsuccessfulness of
EXPERIMENTS.

I Am very sorry, *Pyrophilus*, that to the many (elsewhere enumerated) difficulties which you may meet with, and must therefore surmount, in the serious and effectual prosecution of Experimental Philosophy, I must add one discouragement more, which will perhaps as much surprize you as dishearten you; and it is, That besides that you will find (as we elsewhere mention) many of the Experiments publish'd by Authors, or related to you by the persons you converse with, false or unsuccessful, (besides this, I say) you will meet with several Observations and Experiments, which though communicated for true by Candid Authors or undistrusted Eye-witnesses, or perhaps recommended to you by your own experience, may upon further tryal disappoint your expectation, either not at all succeeding constantly, or at least varying much from what you expected.

This Advertisement may seem of so discouraging a nature, that I should much scruple the giving it you, but that

I suppose the trouble at that unsuccessfulness which you may meet with in Experiments, may be somewhat lessened, by your being forewarned of such contingencies: And that if you should have the luck to make an Experiment once, without being able to perform the same thing again, you might be apt to look upon such disappointments as the effects of an unfriendliness in Nature or Fortune to your particular attempts, as proceed but from a secret contingency incident to some experiments, by whomsoever they be tryed.

But because, *Pyrophilus*, the Advertisement which I am about to give you may seem, as Paradoxical, so discouraging; it will be but reasonable that I present you with some instances of the requisiteness of it: which I shall the more willingly do, because thereby I may not only evince the truth of it, but somewhat lessen the despondency it is apt to produce, by letting you see, that though some of your Experiments should not always prove constant, you have divers Partners in that infelicity, who have not been discouraged by it.

To make nice and curious distinctions of the several grounds and occasions of the Unsuccessfulness of Experiments, would perhaps prove a work of greater difficulty than use, and therefore I shall content my self grossly to distinguish the causes of that unsuccessfulness, into the particular or mistaken properties of the Materials imploy'd about them, and some such error committed in the handling of these Materials, as though it hinder the success of the Experiment, is not easie to be discerned. Which therefore I mention, that I may distinguish these kind of Errors that I am now to consider from those more obvious ones, which proceeding barely from the unskilfulness of the Tryers of the Experiments, may be easily enough discerned, and either rectifi'd or avoided by a knowing Artist, or a person well vers'd and expert in making those parti-
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cular Experiments, which through that unskilfulness may have miscarried.

The Materials to be employ'd about the Experiments we are considering may also admit of several distinctions; as into Natural and Factitious, Sincere and Adulterate, Simple and Compound, &c. But we shall likewise purposely forbear the insisting on any of these, and content our selves to cast what we have to say on this part of our Theme, into a few and comprehensive Observations.

And in the first place we will observe, that divers Experiments succeed not, because they were at one time tryed with Genuine Materials, and at another time with Sophisticated ones: and in this case it may be all one, as to the Event of the Experiment, whether the Materials wherewith it was successfully try'd were sophisticated or not, if those made use of in the latter tryal were of differing Qualities from those employ'd in the former; because it may very well happen, that sophisticated Bodies (as we may have occasion to shew hereafter) by the addition of those things, or by that deceitful way of preparation, whereby they have been sophisticated, may acquire an aptitude to produce such effects, as had they not been adulterated, they would not have been fit to do. Now it is scarcely imaginable to him that has not been very conversant with the Drugs and Simples sold in Shops, how generally they are adulterated by the fraudulent avarice of the Sellers, especially if they be such whose preciousness may make their Sophistication very beneficial to them that practise it. It has been lately much complained of by some of the Cultivators of *Clower-grass*; that of a great quantity of the Seed not any Grass sprung up; which not being imputable to the Soyl, nor the Sower, proceeds, as some Analogical observations make me suppose, from the effeteness (if I may so speak) of the superannuated Seed sometimes sold in the Shops. And upon this Subject I cannot conceal
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from you what was lately affirm'd to me by one of the eminentest and soberest Chymists of *Amsterdam*, who was also an Indian Merchant, who assur'd me, that most of the Cinnamon and Cloves that is brought into these Western Regions, is defrauded in the *Indies* of much of the finest and subtilest Aromatical parts before it be sent into *Europe*. And to give a more familiar Instance to our present purpose, you may be pleased to remember, *Pyrophilus*, that in one of the first of these Essays, we have made mention to you of great store of living Creatures which we had observed in Vinegar; of the truth of which Observation we can produce divers learned and severe Witnesses, who were not to be convinced of it till we had fully satisfi'd them by ocular demonstration: and yet, *Pyrophilus*, there are divers parcels of excellent Vinegar, wherein you may in vain seek for these living Creatures: and we are now distilling some of that Liquor (which if we did not think to be of the strongest and best sort, we should scarce think worth the being distill'd for Spirit) wherein nevertheless we can neither by Candle-light nor by day-light discern Any of those little Creatures of which we have often seen Swarms in other Vinegars. Of such fraudulent tricks as those lately mention'd, I could easily give you divers Instances, if I were not afraid of teaching Fallacies by discovering them. But some are so notorious, or otherwise of such a nature, as that it may be more useful than dangerous to mention them.

It is commonly known, that *Sublimate* is wont to be sophisticated with *Arsnick*: and how differing the effects of such *Sublimate* may be from those of that which is faithfully prepar'd, not only upon Metals, but (when *Mercurius Dulcis* and other Preparations are made of it) upon humane Bodies, they, and scarce any but they, who are acquainted with the noxious qualities of *Arsnick*, both to Metals and Men, can readily imagine. And indeed as for
 Chymical

Chymical Preparations *Helmont* was not much in the wrong, when he affirm'd, There were scarce any, vulgarly sold in shops, to be rely'd on as faithfully prepar'd. And for my part, I have so often met with Chymical Preparations which I have found unsincere, that I dare scarce trust any, either in the administration of Physick, or so much as in the tryal of considerable Experiments, which either my own Furnaces do not afford me, or wherewith I am not supply'd by some person of whose skill and faithfulness I have a good opinion. The other day, having occasion to use some Spirit of Salt, whereof I was not then provided, I sent for some to a Chymist, who making it himself, was the likelier to afford that which was well made: but though I gave him his own rate for it, at the first Rectification even in a Retort, a single pound afforded us no less than six ounces of phlegm; and afterwards being further rectifi'd in a high body and gentle heat, the remaining Spirit parted with a scarce credible quantity of the like nauseous liquor, and after all these sequestrations of phlegm was not pure enough to perform what we expected from it. Of which complaining to an excellent Chymist of my acquaintance, he sent for Spirit of Salt to a very eminent Distiller of it, who gets much by his profession, and passeth for a very honest man: but this Spirit, besides its weakness, discover'd it self to be sophisticated with either Spirit of Nitre, or *Aqua fortis*, which betray'd it self by its peculiar and odious smell; whereas Spirit of Salt skilfully and sincerely drawn, is commonly of a greenish colour, bordering upon yellow, and hath usually a Peculiar, and sometimes (as I can exemplifie to you in some of mine) a not Unpleasing smell. And let me on this occasion advertise you, *Pyrophilus*, that in divers cases

Accipe pulverem Johannis de Vigo propria manu paratam, uam alioquin admistus Minio est adulteratus, prout quaecunque Medicamen Chymicum quod venale exstat fraude plenum est. Helmon. de Febr. c. 14.

Sunt nempe olea essentialia venalia, quaeque magno ære penduntur, adulterata omnia atque singula, &c. Idem de Febr. c. 15.

'tis not enough to separate the aqueous parts by Dephlegmation, as many Chymists content themselves to do, but some Liquors contain also an unsuspected quantity of small corpuscles of somewhat an earthy nature, which being associated with the saline ones, do clog or blunt them, and thereby weaken their activity: And therefore such Liquors to be well depurated require the being distill'd off, and that with a slow fire, that the dry fæces may be left behind in the bottom of the Glass. To satisfy some persons that this Observation is not groundless, we have sometimes taken of the better sort of Spirit of Salt, and having carefully dephlegm'd it, remov'd it into lower Glasses, (that the less heat will suffice to make the Liquor ascend) and having gently abstracted the whole Spirit, there remain'd in the bottom and the neck of the Retort whence 'twas distill'd, so great a quantity of a certain dry and stiptical substance, for the most part of a yellowish colour, that it seem'd strange to the beholders, that so clear a Spirit should conceal so much of it: and we our selves should have wonder'd at it too, had we not remember'd, that in what the Chymists are wont to call the Oyl or Rectifi'd Butter of *Antimony* made with *Sublimate*, the Liquor, though distill'd and very limpid, almost like fair water, consists in great part of the very body of the *Antimony*: which we would here manifest, but that we elsewhere do it, and therefore chuse rather in this place to take notice, that the Spirit of Salt after this second depuration was so chang'd, that it seem'd to be a much nobler, and almost another Liquor than it was before.

But to return to our sophisticated Spirit: what differing effects would be produc'd by true Spirit of Salt, and that which is mixt with the Spirit of *Nitre*, he that knows the great disparity in the operations of those two Liquors, whereof (to mention now no other Instances) the former will precipitate Silver, when the latter has dissolv'd it, may easily

easily inform you. Which Instances I mention not as the considerablest which may be produc'd on this Subject, but as the freshest in my memory.

In the next place, *Pyrophilus*, I observe, that even when the Materials employ'd about Experiments are no way sophisticated, but genuine, and such as Nature has produc'd them, or Art ought to prepare them, even then, I say, there may be a very considerable Disparity betwixt Concretions of the same kind and name, and which pass without suspicion for bodies of perfectly the same nature.

This may, to you, *Pyrophilus*, seem a great Paradox; but perhaps upon examination it will appear a great Truth: which because I am perchance the first, or one of the first, that has solemnly asserted, I hope I shall obtain your pardon if I insist somewhat the longer upon the making it out. For though *Antimony* (and the like is to be understood of Quick-silver, Gold, Copper, Tin, &c.) is wont by almost all men without hesitancy to be look'd upon as being all of it of the same nature as well as denomination; yet he that will take the liberty to suspect that they may be deceiv'd in that opinion, and then heedfully observe the differing progress and event of Experiments, may very well discern, that there is as well a difference in Minerals of the same kind, as there is in Vegetables and Animals of the same species. And as the White-rose, the Red-rose, and the Damask-rose differ much from one another, though all three be Roses; and as the sowre and sweet Orange are very differing betwixt themselves, and both of them from the *China* Orange, though all be Oranges; and as the Hound, the Grey-hound, the Spaniel, the Tumbler, the Mastiff, and the Water dog, &c. are very diversly qualifi'd, though all of them be Dogs: so neither are all the parcels of *Antimony* to be met with in Mines or Shops of altogether the same Qualities, though all of them be Antimonial Concretes. There is indeed this difference

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betwixt

betwixt the variety to be observed in Vegetables and Animals, and that which is to be found in Minerals, That the former is wont to be more obvious to the Eye, and betray it self by some difference to be observed, either in the size of the Creatures of the same kind, or in some peculiar shape or colour, by which 'tis easie for Nature conspicuously to discriminate bodies that consist of many discernably distinct parts; whereas Minerals appearing to the eye either to be perfectly similar, as Metals, or at least to consist but of two or three distinct ingredients, as *Cinna-ber*, and some other Mineral Concretions, the diversity to be found betwixt Minerals of the same Denomination is hardly to be discerned, before Experience have discover'd it.

And on this Subject I consider, that the womb (if I may so speak) of a Mineral body is not always like that of an Animal, a place by a competent and peculiar involving fence secur'd from the intrusion of all bodies not of kin to that included in it: But a Mineral being generated in the bowels of the Earth, its womb is oftentimes accessible and open to other Mineral Juyces or Steams that pass that way, though of never so differing natures from that of the more copious Mineral. In somuch that not only I have had the opportunity to observe (not without some wonder) Minerals of differing kinds, as *Marchasites* and *Metals*, *Marchasites* and *Stones*, (I mean *Stones* properly so call'd) *Salt* and *Sulphur*, and the like, blended in the same small lump of matter; but I have sometimes found in a great mass of one sort of Mineral, small parcels of a Mineral of a quite differing kind perfectly inclos'd in the substance of the other. But to resume what we were saying before, these intruding bodies (if I may so speak) being coagulated, and perhaps ripened together with the former by length of time, are not easily either separable, or so much as distinguishable at their first digging out of the ground, and much less after
their

their Colliquation. For the ignorant or heedless Mineman aiming only at the obtaining a quantity of such a Metal, or other Mineral, as may be vendible under such a determinate name, has neither the design, nor perhaps the skill, to make nice separations of the Heterogeneous bodies to be met with in his Oar, but melts so much of them as he can promiscuously together, and then sells them, not only to the Merchant, but the Chymist, for that Metal or Mineral whose outward form and properties (as colour, consistence, weight, sound, &c.) it has; though that Metal under whose name it passes, be indeed but the predominant Ingredient of the Lump, wherein divers other Minerals may in small quantities lye concealed, and yet upon occasion be discovered by exquisite separations, or discover themselves by unexpected operations, when they meet with bodies fit to act on them, or dispos'd to receive impressions from them.

I was lately visited by an ingenuous Goldsmith of my acquaintance, who complain'd to me, that being wont to buy parcels of Gold brought in small pieces, and as it were sandy corpuscles, from *Guinea*, or some Country of that Coast, though he found it upon all tryals very right Gold, yet was it so very pale, that few but expert Goldsmiths would meddle with it, as fearing it had been some sophisticated Metal; adding, that this exceeding paleness of it sometimes reduc'd him to melt it with very high-colour'd Gold, or to heighten its tincture with that of Copper, to bring it to the colour of ordinary Gold.

The probability of this may be prov'd by what is related by Monsieur *Flacourt*, Governor of the French Plantation in *Madagascar*, who in his newly publish'd History of that Island, speaking of the Metals of it, says, *Il y a bien, &c.* that is, There is certainly Gold among the Inhabitants of *Madagascar*, which has not been brought thither by Foreign Ships: for 'tis not possible

*Histoire de
Madagascar, c. 37.*

sible that such Ships should have left them so much of that Metal as they have ; and besides, it is of a differing nature from that of *Europe*, which they call in this Country *Vou-lamene Voutroia*. He adds, that this Gold, which they call Gold of *Malacasse*, is pale, and is not worth above 10 Crowns, (or about 50 shillings) an ounce ; also, that the *Negroes* affirm, that there are many Mines of it in the Country where it was formerly digg'd ; that there is three sorts of it differing in fineness from each other, and discriminated by the Natives by three peculiar names. But that which he adds as most considerable, is, *That Malacassian Gold is of so very easie a fusion, that it is almost as easily melted as Lead* ; whereas we here find the Gold we deal with to require considerably strong fires, and are wont to cast in *Borax* to facilitate the fusion.

Having upon occasion had the Curiosity not long since to visit some Mines of Lead, and other Metals, I find, that there is a great difference, and discernable even to the eye, betwixt the several Oars ; for instance, of Lead, some of which I can shew you so like Steel, and so unlike common Lead-Oar, that the workmen upon that account are pleased to call it Steel-Oar, which being of more difficult fusion than ordinary, they are wont to mix it with other Oar, which they call Firm-Oar, to facilitate the melting of it. And I likewise took notice of an Oar, which for its aptness to Vitri-fie and serve the Potters to glaze their earthen Vessels, the Miners call Pottern-Oar, and sell it (at least where I saw it digg'd up) dearer than other Oar, from which it differs both visibly enough, and as the workmen affirm, in divers other (and those less obvious) Qualities ; and yet all these Oars after fusion do pass indiscriminately under the name and notion of Lead. In which therefore it is no wonder that severer Inquirers find a great deal of disparity. I remember I did not long since cause some Lead-Oar to be try'd, which being the most promising

promising that ever I saw, made me suppose it might contain some considerable quantity of Silver: but though it prov'd so rich in Lead as to yield after the rate of seventy pound in the hundred, yet one of the most expert Artists in *Europe* could not extract one grain of Silver out of it; whereas the Lead of very many Mines, being skilfully examin'd, will leave behind it upon the Test a proportion of pure Silver. And though this quantity of Silver be not considerable enough to make such Mines as yield it pass for Silver-Mines, (or, as we are wont to call them, Mines-Royal) because the Silver will not quit the cost of extracting it; yet such Mines, though they pass but for Lead-Mines with the Metallist, may appear to be mixt Mines to the Naturalist, who may meet with divers Experiments, wherein the little Silver that is in them may make their Lead operate differently from that of those Oars which are wholly destitute of Silver.

And as this disparity is discernable in Lead-Oars, so it may well be supposed that the like would be discovered in the Oars of other Metals, if they were but purposely and skilfully examin'd. On which Subject I remember, that a very experienc'd person in these affairs, and otherwise very candid and sober, was lately very desirous I should procure him some Tin-Oar, alledging, that he had met with a sort of it, which after a long digestion in Lixivate Liquors, afforded him a very considerable proportion of the richer Metals, insomuch that having a large quantity of that Oar, and finding the Experiment on it to succeed constantly, he promised to himself a vast Income by it: But when that stock of Oar was spent, the next that he procured, though with great carefulness manag'd as the former, would by no means be brought to afford either so considerable a benefit, or so much as any at all. Which brings into my mind, that having once bought a parcel of block Tin, (as the Trades-men call that which is of the
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most pure or unmixt, and as yet unwrought) I was desirous to try if I could not make a Menstruum to dissolve it in such manner as *Aqua fortis* dissolves Silver, and *Aqua Regis* Gold; because Chymists are wont to complain, that though they have a Menstruum or two that will dissolve crude Tin, yet they want one that will keep it dissolv'd, and will not, which *Aqua fortis* will, let it fall into a Calx. Having therefore (by a way that I elsewhere teach) prepar'd such a Liquor as was desir'd, I evaporated a Solution of the fore-mentioned Tin, and setting it to shoot, found, somewhat to my wonder, that the Crystals it afforded were not at all like any kind of Vitriol, but broad, flat, and exceeding thin, just like those of Silver. Whereupon for further tryal having examin'd this Salt by the Tongue, we found not that it had any such taste as the skilfully made Calx of Tin in Spirit of Vinegar, (wherein 'tis not Every Calx of *Jupiter* that is soluble) which (the last time we try'd) seem'd to us to have, as it were, a chalybeate taste, but such an excessive bitterness as may be met with in the Crystals of Silver made with *Aqua fortis*: Finding also this further resemblance betwixt the Salts of these two Metals, that they did both of them presently dye upon the nails and skin a blackness that could not in a short time be wash'd off: we should have suspected, that the Menstruum had exalted the Metal dissolv'd in it to a greater cognation to Silver; but having afterwards prosecuted the same Tryal with the same Menstruum, and another parcel of block Tin, (the former being casually lost) this Metal, though bought very soon after the other, and, as I remember, at the same place, made us conclude, that the event of our tryals proceeded from our having lighted upon a lump of Tin that was of a peculiar Nature.

I remember also that a while since a learned and inquisitive friend of mine found in his Land a parcel of Oar, part
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of which he shewed me, and some of which I can shew you, but have not yet made tryal of it, which seem'd to me, among others that look'd upon it, to be Copper-Oar, and which did indeed after fusion yield very good Copper; but the persons to whom he committed the examination of the Mine, being very inquisitive, and extraordinarily skilful, they did (as one of themselves immediately after confess'd to me) find in that Oar, besides the Copper, no inconsiderable quantity of Silver; and in that Silver (having dissolv'd it in *Aqua fortis*) a considerable proportion of Gold.

But to detain you no longer on this Subject, give me only leave to strengthen the Paradox I have propos'd by the authority of that great and candid Chymist *Basilius Valentinus*, who speaking of Antimony, after he hath told us that there are several kinds of it, and especially two, the one more Mercurial, and of a Golden property, witnessed by the shining streaks or beams it abounds with, the other more full of Sulphur, but destitute of the golden nature that enriches the former, adds, that there is such a different goodness betwixt the several sorts of Antimony, as there is betwixt the several sorts of Flesh or Fish, which, though agreeing in name and, if you please, in nature, do exceedingly differ in point of goodness. Which brings into my mind the great difference which I have found, even visible to the eye, betwixt the several sorts of Antimony; and makes me also remember, that the other day being by an excellent Chymist shew'd a parcel of Antimony as a rarity, upon the score of the variously-colour'd Sulphur wherewith it was conspicuously enrich'd, the possessor of it soon after employ'd it to make Butter of Antimony: but though he were very expert in that kind of distillation, yet instead of the Liquor he expected, upon the approach of a gentle fire, he found the neck and body of his Retort lin'd with an Antimonial Cinnaber, (or at least a red substance, by him concluded

concluded to be Sulphur) at which being surprized, he was pleased to withdraw his fire till he had acquainted me with this accident, and in the yet unbroken Retort shew'd me the Cinnaber, which is not wont (as you know) to arise till after the Butter of Antimony is come over, and the remaining matter be urg'd with a vehement fire. And 'tis perhaps to the undiscern'd difference of Antimonies that we may sometimes ascribe that contingency, which we have divers times had occasion to take notice of in the making of Antimonial Cinnaber: for though in our Furnaces it hath been very successfully made, yet not only we have afterwards fail'd of making it, but we have seen much more expert Chymists, and who because of the high value they do (not undeservedly) place upon that Medicine, imploy themselves oftner than we in making it, divers times unsuccessfully attempt the preparing it. And it may be perhaps also from some diversity either in Antimonies or Irons, that eminent Chymists have (as we have observed) often failed in their endeavours to make the Starry Regulus of Mars and Antimony. Infomuch that divers Artists fondly believe and teach (what our Experience will not permit us to allow) that there is a certain respect to Times and Constellations requisite to the producing of this (I confess admirable) body. Upon which Subject I must not omit to tell you, that a while since an industrious acquaintance of ours was working on an Antimony, which unawares to him was, as we then suppos'd, of so peculiar a nature, that making a Regulus of it alone (without Iron) the common way, (for his manner of operation I enquir'd of him) he found, to his wonder, and shew'd me his Regulus adorned with a more conspicuous Star than I have seen in several Stellate Regulusses of both Antimony and Mars. Yet I dare not be too confident that this depended only upon the peculiar nature of that Antimony, because since that, my own Laboratory has afford-
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ed me divers such parcels of Regulus without Mars (some of which I have yet by me very fairly stellated) which though made of Antimony that seem'd (by its various colours) to be more rich than ordinary in Sulphur; yet in regard the Antimony did not constantly afford a Starry Regulus, though by the same person order'd as near as could be after the same manner, it did not so clearly appear to me, whether the differing event of the several Tryals proceeded from the peculiar nature of this or that parcel of Antimony, or from some odd and scarce discoverable circumstance in the management of the operation. But in either case, the mention of these uncertain Events will properly enough belong to our present Discourse.

As in Antimony, so (as I intimated above) in divers other Minerals a considerable diversity may be observed: & I remember I was lately presented with a piece of a Mineral, which to me, as well as to the rest who look'd on it, seem'd to be an ordinary and worthless Marchasite; and yet a Dutch Merchant (a skilful Mineralist) who was the possessor of it, was very industrious to procure a greater quantity thereof, having in some of it, on which he had made Tryals, found a rich proportion of pure Gold. And the same Gentleman whose Copper-Oar I formerly mention'd, digging for more of that Oar, found lately a quantity of red Earth, which by knowing Mineralists was guess'd to be but Bolus, and indeed looked very like it; but being melted with Regulus Martis Stellatus by a skilful Tryer of Metals, it divers times richly recompensed the Examiners curiosity, by affording him many grains of fine Gold: and though I doubt whether this Gold proceeded from the Bolus, or the Regulus melted with it, yet however it may serve for an instance to shew that some Mineral bodies, which pass without dispute for Minerals of such and such a precise nature, may have lurking in them Minerals of a quite other nature, which may manifest
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themselves in some particular Experiments, (wherein they meet with proportionate Agents or Patients) though not in others.

That the Talck which is wont to be employ'd (about Cosmeticks is of very difficult Calcination, is so known a thing to those that have tryed to calcine it, that I have met with good Chymists that have looked on all the Calces of Talcks but as Impostures. Nor indeed have we calcin'd Venetian Talck without some length of time, and much violence of heat. But among many sorts of Talck we have here in *England*, there is one which a moderate fire will in less than an hour reduce into a snow-white Calx, of which I had lately a parcel by me; and some days since I met with another sort of English Talck which I could suddenly calcine even with the flame of a candle. And my experienc'd friend Dr. K. assures me, that out of a German Talck he met with, he did by digesting it in a strong Solution of Alcalizate Salts separate pretty store of good Gold, and might have made it a very gainful Experiment, if all the Talck growing in the same place had been of the same richness. The like almost has been affirmed to me by a Gentleman of Eminency, who told me, That from a certain Talck he had out of *Norway*, he had once drawn a pretty quantity of very good Gold: and it seems indeed, that though some have been pleased to laugh at all attempts of sequestering any thing from any kind of Talck; yet some parcels of that Mineral afford good store of a Tincture, which may for ought I know be of a golden nature. For I remember I have met with a kind of darkish-colour'd Talck (whereof I can yet shew you a piece) which when I cast but into *Aqua Regis*, the Menstruum manifestly work'd upon it, and dissolv'd its colour'd parts in such plenty, that the filtrated Solution pass'd without suspicion among divers knowing Naturalists to whom I shew'd it, for a fair Solution of Gold.

Paracel. de Mineral. Tract. 1.

Paracelsus

Paracelsus himself reckons four kinds of Talck, Red, White, Black, and of that colour which his Interpreter translates Luteous: and perhaps each of these colours comprises several kinds of that Mineral. And therefore that Mineralist did not amiss when he added in the same Discourse, after he had mention'd great variety of Marchasites, Stones, and other Minerals; *Sed & hoc verum est, in terra multa adhuc condi, quæ mihi incognita sunt, sed eadem nec alii norunt. Certum siquidem est, progressu temporis tot tamque varia à Deo adhuc proditum iri, de quibus nemo nostrum nedum unquam somniavit.*

*Paracel.
ibidem.*

'Tis vulgarly known, that there is a great difference between Vitriols that are reputed to be meerly of the same Metal. And not to mention those Vitriols that I have either made or seen, of less usual colours; nor to take notice of the Veins, Slate, and even loose Earth, impregnated with Coperas that I have had: to pass by all this (I say) as for those Vitriol Stones whereof we in *England* are wont to make our Vitriol, I have seen at the chief work where Coperas is made so great a variety of them, (divers of which I have yet lying by me) that I could scarcely believe the workmen when they affirmed them to be all Coperas Stones, and cannot but think it both very likely, that some of them contain other Mineral substances besides Vitriol; and very possible that the saline parts of those stones upon their solution by the Rain, may work upon those other substances formerly concoagulated with them, and thereby imbue some parcels of the Vitriol made of them with qualities other than are essential to the nature of Vitriol, or belong ordinarily to it.

That there is also a difference betwixt those bodies that pass under the general name of common Salt, cannot but be obvious to any Chymist that hath occasion to make accurate tryals on that Subject. And as for those Con-

cretes that pass under the name of Salt-peter, there is probably no small disparity among them: for besides the difference which we have observed and which is obvious enough betwixt good English Nitre, and that which is brought us over from *Barbary*, (which before it is much refin'd abounds very much with an adventitious Salt that tastes much like Sea-salt) besides this I say, those that do use both good European & good East-Indian Salt-peter assure me, they find much difference betwixt them, and give the preference to the latter: and indeed I have often thought I discern'd a considerable difference in the operations of several kinds of Salt-peter even after purification: and probably that sort of Salt-peter which near *London* an ingenious man of my acquaintance does sometimes (but cannot always) make, chiefly out of Sea-salt, hath some differing qualities from that which is drawn the common way out of the Earth. And indeed Salt-peter being but a kind of *Sal terræ*, generated in very differing-qualifi'd parcels of Earth, may probably receive divers qualities from the particular soyl wherein it grows, though these qualities lye concealed and unsuspected under the wonted exterior appearance of Nitre. Which consideration brings into my mind what was lately told me by a very ingenious Gentleman concerning one of the eminentest of our *London* Physitians, who was wont, as this Confident of his assured me, as an excellent secret, to imploy in some of his choice Remedies that peculiar Salt-peter which he had drawn out of the Earth digg'd up in Church-yards.

And such kind of differences would probably in other Mineral bodies be taken notice of, if mens prepossessions did not make them ascribe the variations they meet with in their Experiments, rather to any other cause than the unsuspected difference of the Materials imploy'd about them.

Nor is it only, *Pyrophilus*, among Mineral Bodies of the same

same name that such a diversity is to be found, but if narrowly look'd into, 'tis very probable that a greater disparity may be discovered both among Vegetables and Animals, reputed of the same nature, than hath been yet taken notice of. Herbarists indeed have exercis'd a commendable curiosity in subdividing Plants of the same denomination, and few Naturalists ignore that there are (for instance) many sorts of Roses, and of Apples, which differ widely betwixt themselves, as we see the difference betwixt the Red-rose and the White, betwixt the Crab, the Pippin, and the Pear-main. But besides these differences which are obvious enough to be Registred by Botanick Authors, there may be more undiscern'd ones (which yet may be considerable ones) betwixt the Individuals of the same ultimate subdivision of Plants, arising partly from the temperature of the air, which makes (for example) Senna growing in *England* to differ much from that which is denominatd from *Alexandria*; partly from the nature of the soyl, as is obvious in the change produced in wild Simples transplanted into Gardens; and partly from many other causes which we have not now leisure to insist upon. But we see oftentimes, that one Rose much differs from another of the same kind, and one Pear-main from another Pear-main. To which we may add, that the upper crust or surface of the Earth being impregnated with subterranean exhalations of several sorts, and tempered with variety of Juyces, it may very possibly be, that some particular Plant may attract some such Juyce out of a determinate spot of ground, as may give it Exotick qualities, and make it differ even from the neighbouring Plants of the same kind. To which purpose I remember, that travelling divers years since from *Geneva* towards *Italy*, I was in my passage through *Switzerland* by a Gentleman of those parts (whose brother had been formerly my Domestick) invited to his Castle, and entertained among other things with a sort of
Wine

Wine which was very heady, but otherwise seem'd to be Sack; and having never met with any such Liquor during my long stay in those parts, I was inquisitive to know whence it was brought: and being answered that it grew amongst those Mountains, I could not believe it, till they assur'd me, that growing on a little spot of ground whose entrails abounded with Sulphur, it had from the soyl acquired its inebriating property, and those other qualities which made it so differing from the Wine of the rest of the Vineyards of that Country. And now I mention Wine, give me leave, *Pyrophilus*, to put you in mind of taking notice what a great change is made in that Liquor, when upon the recess of the spirits and more volatile sulphureous parts, or else the new texture they make with the others, it degenerates into Vinegar, and yet how little either diminution of quantity or any other alteration doth appear upon this change to the beholders eye. And though no body is like to lose an Experiment by mistaking Vinegar for Wine, because both those Liquors and the changes of them are so familiar unto us, and because we are wont to taste each of them before we imploy it; yet who knows what changes there may be in other Bodies with whose alterations we are unacquainted, though the Eye, which is oftentimes the only Sense employ'd about judging of them, discern no change in them? as may daily be observed in the superannuated seeds of Plants, which after their having been kept long beyond their due time, lose all their germinating power without losing any of their obvious qualities. And here let me further observe to you, that Urine is made much use of, not only by Dyers, but several other Trades-men in divers operations (some of which we may elsewhere have occasion to treat of) belonging to their professions. Now these men being wont indiscriminately to employ Urine, without examining whether it be rich in Salt or not, and how long it hath been kept,

kept, it may not be impertinent to take notice that Chymists, who have occasion to distil it often in great quantities, assure me that they find a notable disparity betwixt Urines, that of healthy and young men abounding much more with volatile Salt than that of sickly or aged persons; and that of such as drink Wine freely being much fuller of spiritous and active parts than that of those whose drink is but Beer or Water. But because the differing strength of Urines, though it be very probable, is not so easily to be satisfactorily made out, we shall rather insist on this other Observation confirmed to us by Experience, which is, that though Trades-men are often wont to boil such and such things indifferently in any Urine, as if it were all one how new or stale it is, they may sometimes thereby commit considerable errors. For recent Urine, wherein the saline parts are yet intangled among the rest, will suffer it self to be boil'd above one half or two thirds away, without the avolation of its volatile salt and spirits. Whereas Urine that has been divers weeks kept is liable to a Putrefaction, whereby the Cement (if I may so speak) of the Ingredients that it consists of, perishing, or some change of texture occasioning their disjunction (if not also concurring to produce them) the component parts fall asunder, and the saline Particles extricating themselves from the rest, will even upon a very gentle heat (as a tryal made on purpose has inform'd us) flye away, and leave a phlegmatick and unactive Liquor behind them. In confirmation whereof I must acquaint you, *Pyrophilus*, with what lately befel me in reference to the distillation of Urine: for having caused some of it to be buryed in earthen Vessels in a dunghil to be there putrifi'd, for five or six weeks, I was by divers occasional Journeys kept from employing it, till it had layn there between four and five months; and observing, when I caus'd it to be taken out, that the covers of the vessels had not been, by him I employ'd to put them

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in, well luted on, and besides were in some places crackt, I suspected that the Heat of the Dunghil had not only loosened the saline parts of the Liquor, but driven them away: and accordingly by distilling it in a very gentle heat, and in a very high Cucurbit, we obtain'd instead of an active and saline spirit, a languid and nauseous phlegm. And how great odds there may be betwixt some Experiments made with recent and putrif'd Urine, may be easily conceiv'd by him who knows what operations Salts have in the business of Colours, and is acquainted with their efficacy in those other Mechanical Experiments wherein Urine is wont to be employ'd. But I fear I have dwelt too long upon this Theme, and therefore I shall proceed to the next.

And in the third place, *Pyrophilus*, I shall observe to you, that there is a great difference to be found among many things prepar'd by Art, that pass under the same general name: which difference may proceed partly from that which we have already observ'd to be found in the Materials of which such factitious Bodies are made, and partly from the way us'd in preparing them. To these heads many particulars may be reduc'd: But we shall at present restrain our selves to the mention of two sorts of prepar'd bodies, namely, of such as are not purifi'd and exalted enough, and of such as are so too much.

And to begin with the first of these; it is very certain, that divers Chymical Experiments delivered by sober Authors have been believed false, only because the Menstruums or other Materials employ'd in the unsuccessful tryals of them were not as highly rectifi'd, or otherwise as exquisitely depurated, as those that were us'd by the Deliverers of those Experiments; so that oftentimes the fault of a bad Menstruum is injuriously imputed to a good Artist. That experienc'd Chymist *Van Helmont*, in his *Paradoxical Treatise of the Stone*, endeavors (as we have else-

elsewhere mention'd) to explicate the manner of its being generated by the Coagulation immediately ensuing upon the mixture of the two volatile Spirits of Urine and of Wine. This noble Experiment has been by many unsuccessfully try'd, and has been therefore by them discredited as a Chymical fiction: and indeed the first, and I think the second time we attempted to make that Coagulum, we found nothing at all of any such thing as we expected upon the confusion of the two fore-mentioned Liquors, which though never so much shaken, and afterwards permitted to rest, did never in the least measure coagulate, which made us long suspect the Experiment; till at length our favourable thoughts of that expert Chymist, making us think it possible that the Spirits we employ'd had not been sufficiently exalted, we dephlegmated some by more frequent, and indeed tedious Rectifications (which yet prov'd but necessary) and then were satisfied by more accurate tryals, that *Helmont* had not mis-inform'd us.

So likewise the same Author in his Treatise *de Peste* much extolling, as a friend to the Stomach, the Entrails, the nervous parts, and even the Head, the Tincture or Solution of Amber made with spirit of Wine (which Medicine is indeed no ignoble one when administred to Constitutions that can well bear the heat of it) divers Physitians and Chymists have attempted the preparing of this Tincture with such bad success, that they have given out, that either *Helmont* delivered what was not true, or conceal'd some considerable Circumstance of the Process.

Whereas having digested sufficiently dephlegm'd spirit of Wine upon very finely powder'd Amber, (which if it be the higher-colour'd yields the deeper Tincture) in a very gentle heat, (for the neglect of which Caution even expert Artists have often lost their pains and glasses) we have several times had a good yellow Tincture of Amber, which was discernable in the Menstruum both by the smell

and taste ; and to satisfy some that suspected the Tincture to proceed but from the exaltation of the Menstruum it self by Digestion, and to manifest that it was a real Solution of the subtiler parts of the Amber, we poured some drops of it into Beer, or Water, into which the spirit of Wine suddenly diffusing it self, the dissolved Amber was plainly discernable swimming like a thin film upon the surface of the Liquor, whence little by little it steamed away into the air.

There is likewise, as we have try'd, to be drawn with spirit of Wine from pure Salt of Tartar a pretty high Tincture, and of a taste which I thought not unworthy the taking notice of: but having a while since try'd to draw this Tincture with spirit of Wine which (unknown to me) was much too weak for that purpose, after I had kept the Glass a while in Digestion, coming to look whether or no the Spirit was ting'd, I found that the Salt of Tartar had drawn to it self and imbib'd the aqueous particles of the Spirit of Wine, and being thereby (for a great part of it) dissolv'd into a Liquor like that which is commonly called *Oyl of Tartar per Deliquium*, the subsiding Salt was by the interposition of that saline Liquor protected from the action of the spirit of Wine, which being by this new way dephlegm'd would not mix with the saline Liquor, but swam entirely above it. To which I shall only add in general, that the German Chymists are divers of them so accurate in the Rectification of their Spirit of Wine, that in *England*, where we are wont to be less careful about that Particular, it is usual enough for those Experiments of theirs to be unsuccessfully try'd wherein the Alcohol of Wine (as they call it) is requisite.

And as Spirit of Wine, so many other Menstruums are made unfit for the perfecting of divers real Experiments, barely by their not being sufficiently freed from their weakning Aquosity.

Nor is it only, *Pyrophilus*, in Menstruums, but in divers other Bodies, that the want of an exquisite Depuration may produce in Experiments variety of Events. As for instance, It has been complain'd of by sober men, that their Preparations of Silver, though never so carefully made, have been apt to produce violent Vomits; whereas we have not observ'd a well-prepar'd Medicine of duly refin'd Silver to work Emetically even in Women and Girls, but by Seige or Urine. But we cannot wonder at the violent operation of Medicines made of ordinary Silver: for not only that which is coyned is wont, as the Mint-masters themselves have confess'd to me, to be allay'd with sometimes about a twelfth part, sometimes a smaller or greater proportion of Copper, for the greater conveniency of the Coyn, but even that Silver which is commonly at great rates sold for refin'd Silver, is not wont to be sufficiently freed from its Copper. Which I not long since manifested in the presence of one of our richest and eminentest Refiners, by dissolving some of his purest Silver in his own *Aqua fortis*; for the greenness of the Solution quickly betray'd the adherency of *Venus* to the Silver. And no wonder; for I have seldom seen our chiefest Refiners blow off from their Silver upon the Test above half its weight of Lead, whereas we think not our Silver sufficiently refin'd for some purposes, till it have been freed from five or six times its weight of *Saturn*, and then it has sometimes afforded a Solution almost as clear as water, with only now and then a light touch of Sky-colour, but nothing near so high as the Ceruleous (Liquor that is supposed to be a true) Tincture of Silver, artificially separated from the rest of the Body.

Now that ill effects by the mixture of Copper may be produc'd in such Medicines as ought to be of pure Silver, he that is acquainted with the violent Emetick qualities of *Venus* can scarcely doubt. And as in men's bodies, so in other

subjects, those Experiments may easily deceive the Artists expectation, when he hopes to perform with Silver and Copper together those things which suppose and require Silver without Copper, or any adventitious Metal. And as Silver, so Gold is very often employ'd for pure, when it is not so : for even the foliated Gold which is commonly sold here in *England*, how fine soever 'tis reputed, is not altogether free from the pollutions of other Metals : for our Gold-beaters, though for their own profit sake they are wont to use the finest coyned Gold they can get (as that which is capable of the greatest extension under the Hammer) yet they scruple not to employ coyned Gold, and that the Mint-masters (as themselves inform me) are wont to allay with Copper or Silver, to make the Coyn more stiff, and less subject to be wasted by attrition. And as for those many Gold-smiths and Chymists who think their Gold most requisitely refined when they have blown from it on the Test a due proportion of Lead, they may be therein sometimes mistaken : for though *Saturn* may carry away with him all the Copper that did imbase the Gold, yet he does not likewise free it from the Silver (for which purpose *Aqua fortis* is therefore wont to be us'd) nay, the skilfullest Refiner that I ever yet knew, hath several times affirmed to me, that coupleing fine Gold with Lead, the Gold has after retained and protected from the fire a proportion of Silver that lay lurking in the Lead, and was afterwards separated from the Gold by *Aqua fortis*, but in so small quantity, that the Experiment (the cost and pains considered) was not lucriferous. And of this sort of Instances, *Pyrophilus*, more might be presented, if we did not think Prolixity might be unwelcome to you.

But as many Experiments succeed not according to expectation, because the Menstruums employ'd about them were not pure enough, so some miscarry because such Menstruums

struums are but too exactly depurated : for it is not so much the purity of Liquors in their kind, as their fitness for the particular purpose to which they are design'd, that is in Experiments to be principally regarded. For instance, we have sometimes for recreation sake, and to affright and amaze Ladies, made pieces of white paper and linnen appear all on a flame, without either burning, singeing, or as much as discolouring them. This is performed by plunging the paper very thoroughly in weak Spirit of Wine, and then approaching it to the flame of a candle, by which the spiritous parts of the Liquor will be fired, and burn a pretty while without harming the paper. But if this Experiment be tryed with exquisitely rectifi'd Spirit of Wine, it will not succeed. Of this Phænomenon this plausible reason has been assign'd, that the flame of the Spirit of Wine is so pure and subtile, that like an *Ignis lambens*, it will not fasten upon the paper. But Experience has inform'd us, that this Conjecture is but a mistake, for the flame of Spirit of Wine is so hot, that I have in Lamp-furnaces employ'd Spirit of Wine instead of Oyl, and with the same flame I have not only lighted paper, but candles, and even melted foliated gold. The true reason therefore why that paper is not burned by the flame that plays about it, seems to be, that the aqueous part of the spirit of wine being imbibed by the paper keeps it so moist, that the flame of the sulphureous parts of the same spirit cannot fasten on it. And therefore when the deflagration is over, you shall always find the paper moist ; and sometimes we have found it so moist, that the flame of a candle would not readily light it. And on the other side, having purposely made tryals of plunging paper into sufficiently dephlegmated spirit of wine, the paper not having aqueous moisture to defend it, was very readily kindled and burned by the flaming spirit. And one of our best ways to try the pureness of spirit of wine is grounded on this.

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very supposition : For dipping it in a Cotton-wick like that of a candle, and setting it on fire, if the flame fasten on the wick, it is a sign of the goodness of the spirit ; but if it do not, we conclude it to be weak, and not sufficiently dephlegm'd. It hath been likewise observ'd, that *Aqua fortis* will work more readily on Lead if it be allay'd with water, than if it be purely rectifi'd. I other-where also mention an *Aqua fortis* I have us'd, which was so strong, that it would not well dissolve silver it self unless I first diluted it with fair water. And within this very week wherein I write these things, I have had an unwelcome proof that Liquors may by too exquisite a Depuration be made unfit for our purposes. For having, to gratifie some ingenious friends, made a certain Menstruum, wherewith we had formerly done some things upon Gold which were (not altogether without cause) thought strange enough, we took care at this time to separate it from whatever was either of an aqueous or an earthy nature more exactly than ever we had formerly done. But coming to make use of this sort of Menstruum, we found to our trouble and loss, that instead of performing its wonted operations upon Gold better than before, we could do nothing at all with it : For it will not now by heat it self be brought to touch gold, though that Metal were wont to be dissoluble in it even in *frigido*. And to satisfie you, that 'twas the too-exquisite depuration of this Liquor, especially from its terrestrial parts, that thus unfitted it to touch a Metal which is otherwise wont to melt as it were naturally in it without Ebullition (almost like Ice in luke-warm water) we will subjoyn, that not only we in vain try'd to make it serviceable by weakning it with fair water ; but having, for tryal sake, taken a little of this numerical parcel of Liquor before it was so carefully rectifi'd, we found that it dissolv'd crude gold as well as we had reason to expect. And it would be consider'd whether or no in the Extracti-
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on of the Tinctures of several Bodies, Chymists do not only put themselves to a needless, but to a prejudicial trouble, when they refuse to employ any other spirit of wine than that which is highly rectifi'd. For, though in many Bodies the parts desir'd by the Artists being the Sulphureous ones, the Menstruum is the better for an exquisite Dephlegmation; yet in divers other Concretes the useful and efficacious parts have in them something of Saline, which makes them more free to impregnate copiously such Liquors as have some Aqueous mixed with their Sulphureous parts.

But because there is nothing more easie than by diluting spirit of wine, though never so strong, to make it as weak as one pleases; and because pure spirit of wine is that of all other Menstruums that Chymists generally make most use of, and which costs them most of charge and trouble, (insomuch that here in *London* that which is perfectly dephlegm'd is valu'd, in their shops that sell both, at ten times the price of common spirit of wine;) I presume you will not take it ill, that without being oblig'd to it by the Title of this Discourse, I take this occasion to acquaint you with the way I employ to obtain dephlegm'd spirit of wine: Especially since the practice of the common way of frequent Rectifications is (not to mention other Inconveniences) wont to prove either exceeding tedious, or insufficient. Put then about an inch thick of Tartar calcin'd to whiteness (for I find it not necessary to reduce it to a Salt) and very dry into the bottom of a tall and slender Glass body, and pour on it as much spirit of wine that has been but once rectifi'd, as will, when they have been shak'd together, swim above the Tartar a fingers breadth (more or less in proportion to the Tartar you put in) and then the Head and Receiver being carefully fasten'd on again, in a gentle heat draw off the spirit of wine, shifting if you please the Receiver when about half is come over, and if
need

need be, rectifying once more all that you distil upon dry Calx of Tartar as before. Whether or no you may meet with this Method in some Chymical Books, I know not: But it seems that either it has not been clearly taught, or has been propos'd by suspected Authors, or else among other Processes, by being found in whose company it has been discredited. For the most ancient and experienc'd Distillers I have met with, have either contented themselves to follow the common way of repeated Rectifications, though thereby they lose much time, and much spirit of wine; or else have had recourse to peculiar Vessels of such a height, as besides that they are neither easily nor cheaply to be procur'd, do not, as far as I have hitherto seen, excuse the need of reiterated Rectifications. Whereas, when we consider'd that the fix'd Salt of Tartar readily imbibes Aqueous bodies, and that yet it will not at all mix with pure spirit of wine, it was easie to conclude, that the Phlegmatick part of the spirit of wine would be soak'd up by the Alcalizate Salt, whereby the inflammable part would be freed from it. And accordingly when we proceeded after the manner above prescrib'd, we found that the Liquor that was produc'd upon the first Rectification from the Salt, being fir'd in a warm Silver-spoon, did not leave behind it one drop of Phlegm, or so much as the least moisture upon the spoon, nay, and indeed did indure a severer Examen, to which for curiositie's sake we thought fit to put it. And when the Distillation was carefully made, we found by frequently (for tryal sake) shifting the Receiver, that all the Spirit that ascended was (to sense) equally pure, since that which came up last of all, even till the Calx seem'd to begin to grow dry, by beginning to cleave at the top, did burn all away, as well as that which came over first. And having for further tryal taken out the calcin'd Tartar, and distill'd it with a good fire, it yielded us pretty store of a nauseous and strongly-scented
Liquor,

Liquor, which seem'd to be but Phlegm, both to the taste, and by its not being at all inflammable though carefully try'd. The same Calx of Tartar being kept in some earthen Vessel upon the fire till it be well dry'd, which will require a good heat, may be employ'd more than once in this operation. And 'twas not needlessly that we prescrib'd Bodies tall and slender: For we found not the Experiment to succeed in large and low ones, and much less in Retorts, in which the Phlegm is wont to rise together with the Spirit; yet we found, that provided the distillation were made with a sufficiently mild heat, a Glass, though very broad, and but moderately high, would serve the turn so far, as that the first half that ascended (the other being very weak) prov'd a Spirit that in a silver-spoon would burn perfectly all away. And because white Calx of Tartar is sometimes not so easie to be procur'd, we will add, that we have for tryal sake sometimes substituted Quick-lime, or Salt of Pot-ashes, (made by a single Solution, Filtration, and Coagulation) with no bad success, especially in case of removing the Receiver before the Ascension of the last part of the Liquor, though even that it self has sometimes from Quick-lime come up inflammable enough. And therefore this Alcohol of wine we peculiarly call the Alcalizate Spirit of Wine; and the rather, because *Spiritus vini Tartarizatus*, which perhaps may be thought the properest name for it, is employed by eminent Chymical Writers to signifie a different thing. And a practicable way of making such an Alcaliz'd and pure Spirit of Wine, we thought not unfit to teach you here once for all, in regard the Menstruum is so highly useful, not only for Tinctures, Extracts, and many other Chymical operations, but in the making of divers Philosophical Experiments, and particularly some of those which you may meet with in our Writings. And an eminently ingenious person (but to me a stranger) chancing to get a sight of this Essay, was pleased

to give me thanks for this last part of it, because, though he had very often made use of Salt of Tartar to improve Spirit of Wine; yet he did it before, not to dephlegm the weaker Liquor, but to acuate the strong with the Alkali: Which though I deny not to be a thing feasible, yet (as I told him) unless it be skilfully attempted, the highly rectifi'd Liquor that is poured on, will rather leave some of its most spiritous parts behind, than carry up so fixt a Salt.

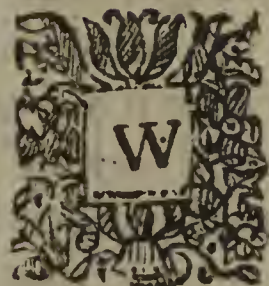
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THE SECOND
ESSAY.

Of Un-succeeding

EXPERIMENTS.



WHAT has been already said, *Pyrophilus*, may, I hope, suffice to shew you, how Experiments may miscarry upon the account of the Materials employ'd in trying them. And therefore we shall now pass on to consider the Contingencies to which Experiments are obnoxious upon the account of Circumstances, which either are constantly unobvious, or at least are scarce discernable till the Tryal be past. And because these Circumstances can hardly be discours'd of in an accurate Method, (which their nature will scarce admit of) I shall not tye my self to any other order in setting down the Instances which occur to me on this occasion, than that wherein they offer themselves to my memory.

And first I must acquaint you with what was not long since seriously related to me by Doctor K. a person exceeding far both from the Custom, and in this particular from the Temptation of telling untruths. He then assur'd me,

that lending his Laboratory in *Holland* to a friend of his during his own absence, and leaving in that Laboratory among other things great store of *Aqua fortis* of several compositions which he had made, to employ about his famous Scarlet Dye, this friend of his sent him word a while after his departure, that by digesting Gold with an *Aqua fortis*, he had separated the Tincture or yellow Sulphur from it, and made it volatile, (the remaining body growing white) and that with this golden Tincture he had, not without gain, turn'd Silver (as to part of it) into very perfect Gold. Upon which advertisement the Doctor speedily returning to his Laboratory, did himself with the same *Aqua fortis* divers times draw a volatile Tincture of Gold, which did turn Silver into true Gold: and (that I may add That upon the by, to gratifie your curiosity) when I demanded whether or no the Tincture was capable to transmute or graduate as much Silver as equall'd in weight that Gold from whence the Tincture was drawn, he assur'd me, that out of an ounce of Gold he drew as much Sulphur or Tincture as sufficed to turn an ounce and a half of Silver into that noblest Metal. Which I am the more disposed to believe, partly because my Tryals permit me not to doubt of the separableness of a yellow Substance or Tincture from Gold, and partly because I am tempted to think, that Silver may have in it a Sulphur (to speak in the Chymists Language) which Maturation is capable to graduate into a Golden one, by having been certifi'd by the observations of men very experienced in Metalline Affairs, (and perhaps too by my own) that sometimes by corrosive Liquors (which Sir *Francis Bacon* also, if I mistake not, somewhere observes) and sometimes by the operation of common Sulphur (especially well open'd and associated with fit Salts) Silver has afforded some grains of very pure Gold. But our Doctor found himself much mistaken in the hopes of growing rich by this Experiment: For a
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while after endeavouring to make it again, his hopes were frustrated, which he ascribes to the *Aqua fortis*, and therefore has attempted the same work afresh. But since all his Tryals have been hitherto fruitless, 'tis not improbable that the disappointment proceeded from some other more abstruse cause; for we find such Adventures to have sometimes befallen Artists irreparably. And *Glauber* alone, if you will therein credit him, tells us of several ways by which he made Gold once, and could not do it again. Upon which Subject I must not omit those very illustrious Testimonies and Instances of this nature, that I find recorded by that Ornament of his Age and Quality, the Prince of *Mirandula*, in his Treatise *de Auro*.

Novi (says he) *qui mihi asseruerit semel se ex mobili argento quod vivum dicitur stabile verumque argentum confecisse succis & foliis herbarum; idque vendidisse peritis explorandæ Metallicæ veritatis; eisdem mox usum se foliis frustra, & quod semel perfecerat, nunquam alias, quanquam id saepe tentaverit, perficere potuisse.* Lib. 3.
cap. 6.

Alium novi (says he further) *qui adhuc apud vivos moratur, cui cum aurum & argentum circiter Quindecies per artem effectum esset, amisit artem eam, accepitque oraculo socii per quietem habito, id ingratae mentis vitio contigisse. Ut hinc etiam veritatem Apostolici dicti condiscamus, Neque qui plantat, neque qui rigat est aliquid, sed incrementum dat Deus.* And to both these Narratives our learned Prince does in the same Book add divers others. *Retulit quidam mihi* (subjoyns he) *se se Aurum ex argento fecisse semel magnâ copiâ; secundo se usum eisdem rebus, fecisse quidem, sed minimâ semper Quantitate, sic ut detrimentum lucro majus esse supputaverit. Venisse in mentem uti detrimentum effugere possit, si non ex argento, sed ex ære melioris conditione metalli, sese consequi experiretur, idque se conjecturis firmis nixum tentavisse: cumque in eo fuisset ut rem sese adepturum speraret, miris modis evenisse, ut nihil omnino consequeretur.* Idem.

Idem (continues the Prince) *affirmavit ab amico qui expertus hoc ipsum fuerat accepisse, qui cum ex Cinnabari argentum fecisset optimum, sepe numero sese postea insistentem operi majore cum diligentia semper eventu rei fuisse frustratum.* And to these Relations of this famous Prince I could add others of some Acquaintances of mine, who having either once or twice made *Luna fixa* (as Artists call that Silver, which wanting but the tincture of Gold abides the tryal of *Aqua fortis*, &c.) or some other Lucriferos Experiment, have since in vain attempted to do the like again, and yet have their eyes so dazel'd by the Gold and Silver they have (either separated or) made, that they are not to be prevail'd with to desist from prosecuting their uncertain hopes.

That diverse Experiments succeed when try'd in small quantities of matter which hold not in the great, it may save you something to be advertised of; diverse Projectors, especially Chymists, having already very dearly bought the knowledge of that truth. For oftentimes a greater and unwieldy Quantity of matter cannot be expos'd in all its parts to a just degree of fire, or otherwise so well manag'd as a less Quantity of matter may be order'd. But this is so manifest a truth to those that have dealt much in Experiments, that whereas many Chymists would be vastly rich, if they could still do in great Quantities what they have sometimes done in little ones, many have undone themselves by obstinately attempting to make even real Experiments more gainful.

I have not been very sollicitous to subjoyn Particulars to the foregoing Observations, because that by reason of the Contingency of such Experiments as would be the most for my present purpose, you might possibly be tempted to lose toyl and charges upon tryals very likely not only to delude your hopes, but perhaps to make you distrust the fidelity of our relations. Yet for Illustration sake of what we have deliver'd, I am willing to mention
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some few contingent Experiments that occur to my thoughts.

And first, it is delivered by the Lord *Verulam* himself, as I remember, and other Naturalists, that if a Rose-bush be carefully cut as soon as it has done bearing, it will again bear Roses in the Autumn. Of this many have made unsuccessful tryals, and thereupon report the Affirmation to be false; and yet I am very apt to think, that the Lord *Verulam* was embolden'd by Experience to write as he did. To clear up which difficulty, let me tell you, that having been particularly solicitous about the Experiment, I find by the relation both of my own and other experienc'd Gardeners, that this way of procuring Autumnal Roses will in most Rose-bushes most commonly fail, but in some that are good bearers it will succeed; and accordingly having this Summer made tryal of it, I find, that of many bushes that were cut in *June* in the same Row, the greater number by far promise no Autumnal Roses, but one that hath manifested it self to be of a vigorous and prolifick nature, is at this present indifferently well stor'd with Damask-roses. And there may be also a mistake in the kind of Roses: for experienc'd Gardeners inform me, that the Musk-rose will, if it be a lusty Plant, bear flowers in Autumn without the help of cutting. And therefore that may be misascrib'd to Art; which is the bare production of Nature. And Cinnamon Rose-bushes do so much better thrive by cutting than several other sorts, that I remember, this last Spring my Gardener having (as he told me) about mid-*April* (which was as soon as that kind of Rose-bush had done bearing) cut many of them in my Garden, I saw about the middle of *June* store of the same bushes plentifully adorn'd both with Buds and with blown Flowers.

An uncertainty not unlike that which we have newly taken notice of in the Experiment of producing Autumnal Roses, has been likewise observed in the attempts that have

have been made to make diverse sorts of Fruit grow upon the same Tree. And as for differing sorts of Fruits of the same denomination, as Apples, Pears, &c. though some severe Naturalists are unwilling to believe that they can be made to grow upon the same Tree; yet we dare not imitate their severity, having lately seen various sorts of Pears fed by the same Tree, and elsewhere three and twenty sorts of Apple-Grafts flourishing upon the same old Plant, and most of them adorn'd with Fruit. Nay, and though the Fruits be not of the same denomination, yet if they be of kin in nature, they may very possibly be brought to grow on the same Tree: for we lately gathered ripe Apricocks and ripe Plums upon one Tree, from which we likewise expect some other sorts of stone-fruit. But to make fruits of very differing natures be nourished prosperously by the same stock, is so difficult a thing, that we can at most but reckon it among contingent Experiments: for though *Pliny* and *Baptista Porta* relate their having seen each of them an example of the possibility of producing on one Tree great variety of differing fruits; and though such a person as the deservedly-famous Astronomer *Dr. Ward* assures me, that he has particularly taken notice of Pears growing upon an Apple-tree; and I elsewhere add a resembling Observation of ours; yet certainly this Experiment has been for the most part but very unsuccessfully attempted, nor have I yet ever seen it succeed above once, though try'd with very much care and industry. And I remember that this very year, in the same Garden where I gather'd the Apricocks and Plums above mention'd, I saw the Ciens of a Pear-tree so skilfully grafted upon an Apple-stock, that it flourish'd very much with blossoms in the Spring, and gave me great hopes that it would bear fruit this newly-past Summer, but has deceived my expectation, as divers other Plants so grafted in the same Garden have for many years deluded the hopes of the

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the skilful Master of it, who assures me, that though divers of them did for some years successively afford promising blossoms, yet they all decay'd away without bearing any of them any fruit. Which yet may seem somewhat strange, since not only we have this Summer gather'd Pears upon a graft which a Divine, to whom the Garden belongs, affirm'd to have been grafted upon a Quince-tree;

and the industrious Kircher tells us, that *Experientia docet Persicum Moro insitum fructus proferre, &c. de quo nullum est dubium utpote*

Artis Mag. Lucis & Umbrae lib. 1. p. 3. cap. 6.

vulgare penè: but experience tells us, that as little as a White-thorn and a Pear-tree seem of kin, a Ciens of the latter will sometimes prosper well being grafted upon a stock of the former.

To contingent Experiments, *Pyrophilus*, you may if you please refer what is delivered by those learned Writers, who affirm, That if a Lixivium made of the Ashes or fixt Salt of a burn'd Plant be frozen, there will appear in the Ice the Idea of the same Plant: For we have divers times purposely made trial of this Experiment without the promised success: and I remember that in the last cold season, proper for such trials, I purposely made a Lixivium of fair Water and Salt of Wormwood, and having frozen it with Snow and Salt after the manner of Congelation else-where declar'd, I could not discern in the Ice any thing more like to Wormwood than to several other Plants; and having about the same time, and after the same manner, expos'd to congelation a thin Vial full of a strong Decoction of Wormwood, (from which an Idea of the Plant may be more probably expected) those to whom I shew'd it after it was frozen could discern as little like Wormwood in it as my self. 'Tis true, that in both these Vials the Ice seem'd somewhat oddly figur'd; but it is true also not only that we have observed that Water wherein a saline body, as Salt-peter, or Sea-salt, or Sugar, &c. has been dis-

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solv'd, has afforded us Ice which seem'd to shoot into several figures, but even in ordinary water congealed we have often seen Ice figur'd, as if the water had been no Elementary body; which needs not be admir'd, since (to omit other causes which may concur to the production of this effect) many Waters gliding through Earths abounding in saline particles of this or that nature, may be easily, in their passage, impregnated with them; whence perhaps it comes to pass, that Dyers find some Waters very fit, and others very unfit for the dying of Scarlet and some other Colours. And therefore we cannot but think that the figures that are oftentimes to be met with in the frozen Lixivium or Decoction of a Plant, will afford but uncertain proofs that the Idea of each, or so much as of any determinate Plant, displays it self constantly in that frozen Liquor. And I much fear, that most of those that tell us that they have seen such Plants in Ice, have in that discovery made as well use of their Imagination as of their Eyes. And 'tis strange to observe what things some men will fancy, rather than be thought to discern less than other men pretend to see. As I remember Mr. R. the justly famous maker of Dioptrical Glasses, for merriment telling one that came to look upon a great Tube of his of 30 foot long, that he saw through it in a Mill six miles off a great Spider in the midst of her Web; the credulous man, though at first he said he discern'd no such thing, at length confessed he saw it very plainly, and wonder'd he had discover'd her no sooner. But yet, *Pyrophilus*, because two or three sober Writers do seriously relate some stories of that nature upon their own observation, I am content for their sakes to reckon their Experiments rather among the Contingent than the absolutely false ones: for it is not impossible but that among the many figures which frozen Liquors do sometimes put on, there may appear something so like this or that Plant, that being look'd upon with the favourable

favourable eye of a prepossess'd beholder, it may seem to exhibit the Picture of the calcin'd Vegetable: and we our selves not very long since, setting to freeze in Snow and Salt a fine green Solution of good Verdegrease, (which contains much of the Saline parts of the Grapes coagulated upon the Copper by them corroded) obtain'd an Ice of the same colour, wherein appear'd divers little figures, which were indeed so like to Vines, that we were somewhat surpriz'd at the Experiment; and that which encreas'd our wonder was, that another part of the same Solution being frozen in another Vial by the bare cold of the air, afforded us an Ice angularly figur'd, (as we have observ'd the Ice of saline Liquors oftentimes to be) but not at all like that made by the application of Snow and Salt. And having for further trial sake suffered that Ice wherein the Vines appear'd to thaw of it self, and having then frozen the Liquor a second time in the same Vial, and after the same manner as formerly, we could not discern in the second Ice any thing like that which we had admir'd in the first. And in Wine and Vinegar, as much as those Liquors partake of the nature of the Vine, we have not after Congelation observed any peculiar resemblance of it in figure.

The mention we have been making of Ice brings into my memory another Experiment, which may perhaps be reckon'd likewise among Contingent ones, and that is the Experiment of burning with Ice as with a Glass Lens; which though some eminent Modern Writers prescribe to be done without taking notice of any difficulty in it, yet both we and others that have industriously enough try'd it, have met with such defeating circumstances in it, especially from the ununiform Texture wont to be met with in most Ice, that the making of such burning-Glasses may be well enough referr'd to those Experiments whose constant success is not to be rely'd on, as we else-where more particularly declare.

In the Trade of Dying there is scarce any tinging Ingredient that is of so great and general use amongst us as Woad or Glaſtum; for though of it ſelf it Dye but a Blew, yet it is us'd to prepare the cloath for Green and many other of the ſadder Colours, when the Dyers have a mind to make them permanent and laſt without fading: but yet in the decocting of Woad to make it yield or ſtrike its colour, there are ſome critical times and other circumſtances to be obſerved; the eaſie miſtake of which oftentimes defeats the Dyers expectation to his very great loſs, which ſometimes he knows not to what to impute, of which I have heard ſeveral of them complain. And therefore divers of our leſs-expert Dyers, to avoid thoſe hazards, leave off the uſe of Woad, though growing plentifully enough here in *England*, and inſtead of it employ Indico, though it coſt them dearer, as being brought hither ſometimes from *Spain*, ſometimes from the *Barbadoes*, and oftentimes even from the *East-Indies*.

Our *London* Refiners, when to part Silver and Copper they diſſolve thoſe mixed Metals in *Aqua fortis*, are wont afterwards to dilute the glutted Menſtrum with ſtore of fair water, and then with Copper Plates to ſtrike down the diſſolv'd Silver. But becauſe by this manner of proceeding much Copper is wont after the ſeparation of the Silver to remain in the Menſtrum, as may appear by its high tincture, that this thus impregnated Liquor may be improv'd to the beſt advantage, they are wont to pour it upon what they call Whiting (which is ſaid to be a white Chalk or Clay finely powder'd, cleans'd, and made up into Balls) wherewith the tincted parts incorporating themſelves, will in ſome hours conſtitute one ſort of Verditer fit for the uſe of Painters, and ſuch other Artificers as deal in Colours, leaving the remaining part of the Menſtrum an indifferently-clear Liquor, whence they afterward by boiling reduce a kind of Salt-peter fit with the addition of Vitriol (and

(and some fresh Niter) to yield them a new *Aqua fortis*.

And these things I mention, *Pyrophilus*, that you may know what I mean when I tell you, that sometimes the Refiners cannot make this Verditer for a great while together, and yet cannot tell whence their disability to make it proceeds. Of which Contingency I remember I lately heard one of the eminentest and richest of them sadly complain, affirming, that neither he, nor divers others of his Profession, were able, not long since, to make Verditer for divers months together; and that several others were yet at a loss in reference to that particular: though for his part he had, without knowing the Cause of this Contingency, found a Remedy for it, namely, to warm the Menstruum well before it be poured on the Whiting, on which, when the Liquor was warm, the tinted parts would fasten, though they would not, whilst (according to the custom of Refiners) it was poured on cold.

Making likewise the other day a visit to the chief Copperas work we have in *England*, one of the Overseers of it, who went along with me to shew me the contrivance of it, assured me, that divers times, by the mistake or neglect of a circumstance in point of time, they had lost, and are yet subject to lose, some thousands of pounds of Vitriol at a time, which in spite of their wonted, but not sufficiently attentive and skilful care, would degenerate into an Unctuous Substance, not to be reduc'd into good Vitriol again; unless by the tedious way of throwing it abroad, and exposing it with the unprepared stones, from which they draw their Vitriol, to the Rain and Sun to be open'd anew, and fitted for the yielding of Vitriol after the same manner with those crude Minerals.

Upon this occasion I must not omit, because much conducing to the scope of our present Discourse, a memorable Relation that I have met with in the Indian History of the learned *Josephus Acosta*, who diligently survey'd the famous
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and almost inestimable Mines of *Pern*, and (for one that was not a Chymist) has delivered divers considerable and judicious Observations about them. That which I am now to mention is in that Chapter where he treats of the Silver of the *Indies*, set down in these words : It is strange to see not only the difference betwixt the refining of Metal by Fire, and without it by Quick-silver, but also that some of these Metals which are refined by the fire, cannot well be molten with any Artificial Wind, as with Bellows, but when it is kindled and blown with the Natural Air or Natural Wind. The Metal of the Mines of *Porco* is easily refined with Bellows; and that of the Mines of *Potozi* cannot be molten with Bellows, but only by the breath of their Guayars, which are small furnaces upon the sides of the Mountains, built expressly where the Wind lies, within the which they melt this Metal : and though it be hard to yield a reason of this difference, yet it is most certain and approv'd by long Experience.

If there be any Trade that obliges the Artificers to be assiduously conversant with the Materials they employ, it is that of the Glass-men ; and yet even to them, and in their most ordinary operations, there happen now and then little accidents, which though they know not well to what to ascribe, are yet capable of hindring them from doing sometimes what they have done a thousand times. And I remember that among the last times that I have been at a Glass-house, an eminently-skilful Workman, whom I had purposely engag'd to make some Vessels for me that required more than ordinary dexterity, was not able when I came thither to make Metal (as they call that colligated mixture of Sand and fixt Salt whereof they blow their Glasses) tolerably fit to be employ'd : Wherefore he desired me to take the pains to come again another day, and he would try to repair his unluckiness. But the next time

time I came, though it were upon appointment, his Metal prov'd again unserviceable, and instead of being colourless when it was cold, look'd as if it had been stain'd with Blew and Yellow, and was besides britler than it ought to have been. So that it need be no such wonder, if Philosophers and Chymists do sometimes miss of the expected Event of an Experiment but once, or at least but seldom try'd, since we see Tradesmen themselves cannot do *always*, what, if they were not able to do *ordinarily*, they could not earn their bread.

It is affirmed by *Helmont* and others that treat of the *Lapides Cancrorum*, that they grow within the skulls of those Craw-fishes from whence they have their name: but I have known good Anatomists complain, that they have sought them in vain in the heads of those fishes, which may well make them distrust the veracity of those that ascribe them to that sort of Animals; yet we have often taken those stony Concretions out of the heads of Craw-fishes. But passing lately through *Hungerford*, a Town famous for the plenty of such kind of fish, we made diligent enquiry concerning their Nature, and were there informed by those that looked to them, that the Concretions above mention'd are to be found in their heads but about that season of the year wherein they shift their shells, and that at other times of the year, several persons had in vain endeavour'd to store themselves with Crabs eyes at *Hungerford*. And indeed, having at the last time of my being there (which was about the latter end of *June*) caus'd divers large ones to be taken out of the water, we found these little stones in the head but of one of them; whereas about a fortnight before, which was near the Summer Solstice, passing by that place, we found in the wonted parts of the heads several such Concretions as to bigness and shape, but so soft, that we could easily crush and discern them betwixt our fingers. And certainly, the mi-
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state of the circumstance of time has much prejudic'd the reputation of many truths: and I remember that *Asellius*, to whose Anatomical fortune the world is so much beholden, ingenuously acknowledges, that he had like to have lost the discovery of the milky veins, because having at first suspected those unlooked for white Vessels, which he took notice of in the Mesentery of a Dog dissected alive, to be some irregular ramifications of Nerves, he was much confirmed in his conjecture by the next Dog he open'd; for having dissected him at an inconvenient distance of time from the Dogs repast, the slender Vessels he looked for being destitute of the Chyle, which is it that makes them conspicuous, did not appear: So that he had lost the benefit of his first lucky observation, had not his Sagacity inclin'd him to suspect, that if a Dog were plentifully fed at a convenient distance of time before his being dissected, the Vessel swell'd with alimetal juices would be the better discernable; whereupon having feasted another Dog some hours before he opened him, he manifestly detected those milky Vessels, whose discovery has since set Anatomists so usefully on work.

But, *Pyrophilus*, not to exceed the limits of an Essay, I must not multiply Instances of the Contingencies of Experiments, but content my self to tell you in general, that in divers Cases such circumstances as are very difficult to be observed, or seem to be of no concernment to an Experiment, may yet have a great influence on the Event of it. If on either of the Extremes or Poles of a good armed Load-stone, you leisurely enough, or divers times, draw the back of a Knife, which has not before receiv'd any Magnetick influence, you may observe, that if the point of the blade have in this affriction been drawn from the middle or Æquator of the Load-stone towards the Pole of it, it will attract one of the Extremes of an equilibrated Magnetick Needle; but if you take another Knife
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that has not yet been invigorated, and upon the self-same Extremity or Pole of the Load-stone, thrust the back of the Knife from the Pole towards the Æquator or middle of the Load-stone, you shall find, that the point of the Knife has, by this bare difference of Position in the blade whilst it pass upon the Extreme of the Load-stone, acquired so different a Magnetick property, or Polarity, from that which was given to the former Knife by the same Pole of the Load-stone, that it will not attract, but rather seem to repel or drive away that end of the Magnetick Needle which was drawn by the point of the other Knife. And this improbable Experiment not only we have made trial of, by passing slender Irons upon the Extremities of armed Load-stones, the breadth of whose Steel-caps may make the Experiment somewhat less strange, but we have likewise try'd it by affrictions of such Irons upon the Pole of a naked terella, and we found it to succeed there likewise. How strange soever it may seem, that the same point or part of the Load-stone should imbue Iron with contrary Properties, barely as they are, during their passing over it, drawn from the Æquator of the Load-stone, or thrust towards it. But whether, and how far this Observation insinuates the operations of the Loadstone to be chiefly performed by streams of small particles, which perpetually issuing out of one of its Poles, do wheel about and re-enter at the other; We shall not now examine (though this seem one of the most likely Phænomena we have met with, to hint a probable Magnetical Hypothesis) contenting our selves to have manifested by what plainly appears, how much influence a circumstance, which none but a Magnetick Philosopher would take notice of; may have on an Experiment. We have also with pleasure observ'd, how Artificers in the tempering of Steel, by holding it but a minute or two longer or lesser in the flame, (or other competent Heat) do give it very

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differing tempers; as to brittleness or toughness, hardness or softness; for as when it is taken out of the flame to be extinguished, it looks either red, yellow, or blew, so they esteem and find it fit to make Knives, Engraving Tools, or Springs for Watches, &c. and yet it passes from one colour to another so swiftly, that none but an Artist expert in tempering of Iron would suspect, that so small a difference of time of its stay in the flame could produce so great a difference in its tempers. On which occasion, *Pyrophilus*, I call to mind, that making a while since some tryals concerning Gravers in the Shop of a famous Artificer, he presented me, as a great rarity, a Graver (which I yet keep) that would make the usual Experiments about tempering of Gravers appear false to him that should never try them but upon it; for with all the care wherewith I try'd upon it the known ways of softning Gravers, I could not soften this: which men eminently skilled in these matters (together with the person that made it) affirmed to have been made of *Damasco-Steel*, the strength whereof in cutting Iron I have (not without some wonder) made trial of. But whether this singularity which we have mention'd in this Graver proceed from the nature of the Steel, or from the temper that it had afterward given it, is not yet agreed upon by those skilful men to whom I have shew'd it: but one of them, who by making Instruments for Navigators, has had the opportunity of making more than ordinary enquiry into matters of this nature, assures me, that he can easily soften this kind of Steel, by only taking it off the Fire at a certain nick of time, differing from that which is wont to be observed in order to the softning of common Gravers. And who knows but that in many other Experiments, seemingly despicable and unheeded Circumstances may be of great concernment, though by reason of the want of such particular Observations as the frequent dealing with the same body has given Magnetick
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Philosophers and Artificers occasion to make, men have not yet taken notice of their importance.

To give you one Instance to this purpose, *Pyrophilus*, let me take notice to you, that divers Planters of Fruit-trees have with wonder observ'd, that some Grafts of Cherry-trees, for example, have born fruit the same year that they were grafted, (nay I have observ'd some Plants to bear fruit the same quarter of the year) and others not till the year after their insition, though neither in the goodness of the Graft, nor in that of the Stock, they had observed any disparity to which the difference above mentioned could be ascrib'd; and therefore the bearing or not bearing of the Ciens of a Cherry-tree the first year of its Insition is by many Gardiners look'd upon as a thing meerly Contingent. And yet indeed it scarce deserves to be reckon'd among such contingent Experiments as we have been hitherto treating of; for I am inform'd by the trials of more than one of the most skilful and experienc'd Grafters of these parts, that a man shall seldom fail of having Cherries born by his Graft the same year in which the Insition is made, if he take care that his Graft, which must be of a good kind, have blossom-buds, as they are wont to be call'd, upon it: Whereas if it were only leaf-buds, as they may be term'd, it will not bear fruit till the second season; and this not being taken notice of by vulgar Gardiners, makes them, as we have said, impute a needless Contingency to the fruitfulness of such kind of Grafts. Now to discern such buds as are fit to produce blossoms from such as will display themselves but in leaves, is no difficult matter, the former sort being more full, and big, and round than the latter, which are wont also to lye more flat and close to the Graft. And 'twas, *Pyrophilus*, such observations as this that induc'd us after the beginning of the former Essays, to discriminate from such contingent Experiments as those wherein the cause of the Contingency

is very abstruse and difficult to be discern'd, such other Experiments whose seeming Contingency proceeds from more easily discoverable causes; for such by diligent observation of circumstances may be reduc'd to a greater certainty than the others seem capable of. Though I dare not deny that even divers of those contingent Experiments, which to us yet seem to belong to the first sort, by mens future skill and diligence in observation may be made fit to be reduc'd to the second sort.

Before I leave this Subject, *Pyrophilus*, I dare not omit to say something to you of the *Virgula Divina*, or rather *Divinatoria*, by which many Mineralists pretend to discover the latent veins of Metals. Some use a forked hazel, whose horns they hold by the ends one in each hand; and others content themselves to chuse a hazel rod (which some will have to be all of the same years shoot) and this they bind on to another streight stick of any other wood, and walking softly with it over those places where they suspect the bowels of the earth to be enriched with Metals, they say, that if they pass over a Metalline vein, the Wand will by bowing towards it discover it. And some Dealers in Metals I know who affirm, that by holding the Metals successively in that hand wherein a man holds the rod, he may discover what determinate Metal is predominant in the Vein: for when he puts into his hand that Metal wherewith the Mine chiefly abounds, the Wand will manifestly bow more strongly than when 'tis held in the hand with any other Metal. What to determine concerning the truth of this perplexing Experiment, I confess I know not. For *Agricola* himself, after a long de-

De re Metall-
câ lib. 20. p. 28.

bate concerning it, gives us this account of his sense, *Metallicus igitur* (says he) *quia eum virum bonum & gravem esse volumus, virgulâ incantatâ non utetur, quia rerum naturæ peritum & prudentem, furcatam sibi usui non esse, sed, ut supra dixi, habet naturalia venarum*

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rum signa que observat. The diligent *Kircherus* informs us in his *Arte magneticâ*, that ha-
 ving exactly try'd the Experiment with Metals, for he mentions not his having try'd it with Mines, he could not find it in any measure succeed; and we our selves having several times made trial of it in the presence of the confidentest assertors of the truth of it, could not satisfy our selves that the Wand did really stand either to the Metals when placed under it, or to the Metalline Veins, when we carried it over Mines whence Metalline Oar was at that very time digging out. But on the other side diverse good Authors, and even our diligent Country-man *Gabriel Plat*, though wont to be somewhat too severe to Chymists, does ascribe very much to this detecting Wand, and diverse persons, in other things very far from credulous, have as Eye-witnesses with great asseverations asserted the truth of the Experiment before us; and one Gentleman who lives near the Lead-Mines in *Somersetshire*, leading me over those parts of the Mines where we knew that Metalline Veins did run, made me take notice of the stooping of the Wand when he pass'd over a Vein of Oar, and protested, that the motion of his hand did not at all contribute to the inclination of the Rod, but that sometimes when he held it very fast, it would bend so strongly as to break in his hand. And to convince me that he believed himself, he did upon the promises made him by his stooping Wand put himself to the great charge of digging in untry'd places for Mines, (but with what success he has not yet inform'd me.) Among the Miners themselves I found some made use of this Wand, and others laugh'd at it. And this I must take notice of as peculiar to this Experiment, that the most knowing Patrons of it confess, that in some mens hands it will not at all succeed, some hidden property in him that uses the Wand being able, as they say, to overpower and hinder its inclinatory virtue. To which I

Lib. 3. part. 5.
cap. 3.

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must add what a very famous Chymist, who affirms himself to have try'd many other things with it besides those that are commonly known, very solemnly professed to me upon his own knowledge, namely, that in the hands of those very persons, in whose hands the Rod will (as they speak) work, there are certain unlucky Hours, govern'd by such Planets and Constellations, (which I confess I believ'd not enough to remember their names) during which it will not work, even in those hands wherein at other times it manifestly will. But of this Experiment I must content my self to say, what I am wont to do when my opinion is ask'd of those things which I dare not peremptorily reject, and yet am not convinc'd of; namely, that they that have seen them can much more reasonably believe them than they that have not.

Nor is it only in Experiments, *Pyrophilus*, but in Observations also, that much of Contingency may be, witness the great variety in the number, magnitude, position, figure, &c. of the parts taken notice of by Anatomical Writers in their dissections of that one Subject the humane body, about which many errors would have been delivered by Anatomists, if the frequency of dissections had not enabled them to discern betwixt those things that are generally and uniformly found in dissected bodies, and those which are but rarely, and (if I may so speak) through some wantonness or other deviation of Nature, to be met with. I remember that a while since being present at the dissection of a lusty young Thief, we had opportunity to observe among other things, that the interval betwixt two of his ribs was near the back-bone fill'd up with a thick bony substance, which seem'd to be but an expansion of the ribs, and appeared not to have grown there upon occasion of any fracture, or other mischance. About the same time being at a private dissection of a large and young humane Body with some learned men, an ingenious Person
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Professor of Anatomy there present, chancing to cut a great Nerve, spy'd in the substance of it a little of a very red Liquor, which he immediately shew'd me, as wondring what it might be : but I concluding it to be Blood, presently suspected that it might have proceeded from some small unheeded drop of blood wip'd off by the brushy substance of the Nerve from the Knife wherewith it was cut. Wherefore carefully wiping a Dissecting Knife, I did in another place cut the Nerve asunder, and found another very little drop of pure blood in the substance of it as before. This I did again elsewhere with like success, shewing it to the by-standers, who admir'd to see a Vessel carrying blood (for such they concluded it to be) in the body of a Nerve, in regard they remember'd not to have ever met with such an accident ; though I the less admire it, because I have in an Oxes Eye or two observ'd in that coat which the Moderns commonly call the Retina, and which seems to be but an expansion of the Pith of the Optick Nerve, little turgent veins manifestly full of blood.

We further observ'd in that lately-mention'd body, in which we took notice of the irregular conjunction of two Ribs, that the Lungs which were very sound had a supernumerary lobe on one side, which did so little differ from its companions, that we did not, till we had display'd the Lungs, take notice of it. And I remember that a while before, being invited by a company of Physitians to a private Dissection, and the Lungs, which otherwise seemed not unsound, appearing in divers places fastned to the ribs, two ingenious Anatomists that were there present, did so little agree in their Observations concerning such cases, that the one affirmed, that he had never seen any Lungs (which had not been excessively morbid) tied to the Thorax ; and the other protested, that he had scarce ever opened a diseased body wherein the Lungs did not so adhere. But if it were not improper to mind a young Gentleman of
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Verereal Observations, I could easily give you an eminent proof of the disagreement of Anatomical Observators, by insisting on the Controversie betwixt the famous Writers on that Subject, concerning the Anatomical notes or tokens of Virginity, many eminent Authors affirming, that they have seldom fail'd of finding them in one amongst very many dissected Maids; and many other Artists, both conspicuous and experienced, peremptorily professing, that they have seldom or never met with the pretended marks in persons even of the most undoubted Virginity: and certainly it is very strange, that about a matter which seems so easily determinable by Sense, accurate and sober men should so widely disagree, as that the one should profess he has very rarely, if ever, met with in a humane body, what another affirmeth himself to have as seldom, if ever, miss'd. But because, *Pyrophilus*, this subject is perhaps somewhat improper to be insisted on either to, or by, a yong man, I shall pass on to tell you, that amongst the accuratest of our modern Writers, I suppose you will readily allow me to reckon *D. Harvey* and *D. Highmore*, and that that though in their excellent Treatises of Generation they both insist on the production and changes observable in Hens Eggs, as the Patterns whereunto the Generation of other Animals may be referred; yet have we diverse times in the progress of Nature in her formation of a Chick, observed considerable variations in point of time and other circumstances (though in the main our Observations commonly agreed) from what is by them delivered: which diversity may easily proceed from the differing constitutions of Hens, their differing assiduity in sitting on their Eggs, the differing qualifications of the Eggs themselves, and several other particulars of like nature. And I remember, that the other day taking notice of this to my learned friend *Dr. Highmore*, he readily acknowledged to me, that he himself had likewise observ'd diverse circumstances

stances in Eggs whilst they were hatching, which varied from those set down by him in his Book, though he had there accurately express'd the changes he discerned in those Eggs which at that time afforded him his Observations. And indeed there are certain things of such a nature, that scarce any single mans accurateness in making a single Observation about them, can secure him from appearing unskillful or unfaithful in his Observations, unless those that shall afterwards examine them chance to be endowed with a somewhat more than ordinary either equity, or sagacity, or both. For instance, he that first affirmed that a Needle animated by a Loadstone did constantly convert its extrems to the opposite Poles of the Earth, could scarce suspect himself of having delivered any thing which he had not carefully try'd. And yet of those excellent Pilots, *Gonzales Oviedo* and *Sebastian Cabot*, (who are said to have in *America* first taken notice of the Declination of the Mariners Needle) he that did first in those far distant parts of the world compare the Meridian Line afforded by Magnetical Needles with one Mathematically drawn (which may be readily found by accurate Sun-dials) and thereby observe the variation of the Needle, or its declination from the true Meridian Line, might easily conclude the Observer formerly mention'd to have been faulty, by reason of his finding the Needles variation differing (perhaps by divers degrees) from that delivered by the *first* Observer. And this *second* mans Observation might appear to have been as carelessly made to a hundred other Observers, if the Observations of Navigators had not made it apparent, that the Declination of the Needle is far from being the same in all places: for though *Cardan* (as *Kircher* and *Fracastorius*, as another informs us) be pleased to affirm, that the Loadstone declines as many degrees as the Pole star is distant from the Pole of the world; yet besides divers

Fournier Hydr.
l.ii. c.ii.

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reasons, common Experience sufficiently manifests the inconsiderableness (not to speak more harshly) of that assertion. For about the Islands of the *Azores*, especially that of *Corvo*, over which the first Meridian is by many supposed to pass, the Magnetick Needle hath bin observed directly to respect the Poles, without any sensible declination from them; but in other places it is wont to vary sometimes Eastward, sometimes Westward, more or less.

much that not only our venturous Countryman Captain *Thomas James* observed it in 63 degrees North-Latitude to be no less than 27 Degrees, 48 Minutes; but a learned Mathematical

In the Table annex'd to his Voyage.

Writer that is lately come forth, makes the Declination at the *Fretum Davis* to amount to what is almost incredible, 50 Degrees. And this Deflexion of the Needle sometimes to one side of the Meridian, sometimes to the other, happens with so much seeming irregularity, as has made both the diligent *Kircher* himself, and divers other Magnetick Writers, almost despair of reducing these kind of Observations to any general Hypothesis.

To which we may add, that perhaps very few even of the exactest Observations of this nature made an Age since, would now appear accurate to them that should try them in the self-same places wherein, and the self-same manner after which they were formerly made. So that the diligentest of those Observers would appear to us to have been negligent, if the sagacity of some of their succeeders had not prompted them to suspect, that even in the same place the Needles variation may vary. And I remember, that having not long since enquired of an English Contriver of Mathematical Instruments for the use of Sea-men, what he had observed concerning this alteration of the Needles variation, he told me, that by comparing of ancient and modern Observations made by himself and other accurate Mathematicians at *London*, he had found the Declination constantly

constantly to decrease, and, as he conjectured, about 12 or 13 Minutes (though that methinks be much) in a year. And it will be yet more difficult to set down any Observation of this nature which will appear exact to posterity, if that strange thing be true (as it may well be) which was related to *Kircher* by a friend of his, who affirms himself to have observ'd a notable change of the Needles Variation at *Naples*, after a great *Incendium* of the neighbouring Mountain *Vesuvius*; which alteration he not absurdly suspects to have proceeded from the very great change made in the neighbouring subterranean parts by that great conflagration. And it seems the same Observation has been taken notice of by Mathematicians elsewhere. For the learned Jesuit *Fournier* in his French Hydrography tells us in more general terms, that since the *Incendiums* of *Vesuvius* the Declination (of the Needle) has notably chang'd in the Kingdom of *Naples*. The same Author somewhere delivers what (if it be true) is remarkable to our present purpose, in these words. There are persons who have observ'd, that the same Needle that declin'd 5 degrees upon the surface of the Earth, being carried down very low into certain Caves, declin'd quite otherwise. I added those words, *if it be true*, not to question the veracity of the Author, but because 'tis very possible the makers of the observation (though learned men) may have been mistaken in it without suspecting themselves in danger of being so. For I should scarce have imagin'd, unless my own particular observation had inform'd me, in how great a variety of Stones and other Fossiles the Oar of Iron may lurk disguis'd: so that 'tis no way incredible, that knowing Chymists themselves, and much more Mathematicians and others, not being aware of the Observation of what I have newly delivered, may presume, because they saw not in the deep Caves above mentioned any Minerals like the vulgar Iron Oar,

that there is nothing of that Metal there, when indeed there may be enough to occasion that Deflexion of the Needle; which (especially if it be strongly excited) may be often drawn aside by Iron or other Magnetick Bodies, at a greater distance than those that have not try'd will be apt to suspect: which may perhaps be the reason why in the little Island of *Ilva* (upon the coast of *Italy*) where they dig up Iron and store of Loadstones, of which I have seen in *Toscany* of a prodigious bigness, there is in different, but neighbouring places, such a strange disparity of the Needles variation as curious men have recorded.

Nor are Magnetical and Anatomical Observations the only ones which are subject to disagree now and then, without the negligence of those that make them: but I want time, and I fear you would want patience, to consider at present as many of them as might be easily enumerated to you.

I suppose, *Pyrophilus*, you may have observed, how I in the past Discourse have forborn to insist on Medicinal Experiments, which I have purposely done, because they are so many, and almost all of them subject to such uncertainties, that to insist on them would require much more time than my occasions will allow me to spend upon this Essay. And indeed in Physick it is much more difficult than most men can imagine, to make an accurate Experiment: for oftentimes the same disease proceeding in several persons from quite differing causes, will be increased in one by the same remedy by which it has been cur'd in another. And not only the constitutions of Patients may as much alter the effects of remedies, as the causes of diseases, but even in the same Patient and the same disease, the single circumstance of Time may have almost as great an operation upon the success of a Medicine as either of the two former particulars, as we may elsewhere have occasion by sundry Instances to manifest. But besides the general uncertainty

to which most remedies are subject, there are some few that seem obnoxious to Contingencies of a peculiar nature: such is the Sympathetick Powder, of which not only divers Physitians and other sober persons have assur'd me they had successfully made tryal, but we our selves have thought that we were Eye-witnesses of the operation of it; and yet not only many that have try'd it have not found it answer Expectation, but we our selves trying some of our own preparing on our selves, have found it ineffectual, and unable to stop so much as a bleeding at the Nose, though upon Application of it a little before we had seen such a bleeding, though violent, suddenly stop in a person, who was so far from contributing by his Imagination to the effect of the Powder, that he derided those that he saw apply it to some of the drops of his blood. Wherefore that the Sympathetick Powder & the Weapon-salve are never of any efficacy at all, I dare not affirm; but that they constantly perform what is promised of them I must leave others to believe. But making mention of remedies of this nature, though I am willing, *Pyrophilus*, to put a Period both to your trouble and my own, yet I must not omit to tell you, that whereas the *Peony-root* has been much commended both by ancient and modern Physitians of no mean account, as an Amulet against the Falling-sickness, and yet has been by many found ineffectual, we have been apt to suspect, that its inefficacy, if it be but infrequent, might possibly proceed from its having been unseasonably gather'd; and when I was last in the West of *Ireland*, acquainting the eminentest of the Galenists there with my Conjecture, he confirm'd me in it, by assuring me that he had often try'd the *Peony-root* unseasonably gather'd without success, but having lately gather'd it under its proper Constellation, as they speak, (which is when the decreasing Moon passes under *Aries*) and ty'd the slit Root about the Necks and Arms of his Patients,

Patients, he had freed more than one, whom he nam'd to me, from Epileptical fits. Agreeable whereunto I find, that a famous Physitian of *Grenoble*, *Monsieur des Grands Prez*, in the last of his Observations communicated to the famous Practical Physitian *Riverius*, solemnly professes his having divers times freed his Patients from the Falling-sickness by the single outward application of *Pæony*-roots, collected and apply'd as is above-mention'd. But though he thence infers the usefulness of observing the Stars in the practice of Physick, yet before much weight be laid upon such improbable Notions as most of those of judiciary Astrologers, the Influence of Constellations upon Simples, &c. ought by severe and competent Experiments to be better made out than hitherto it has been.

But to say no more of the contingent Observations to be taken notice of in tryals Medical, I could tell you that I have observed even Mathematical Writers themselves to deliver such Observations as do not regularly hold true. For though it hath been look'd upon as their priviledge and glory to affirm nothing but what they can prove by no less than Demonstration; and though they use to be more attentive and exact than most other men in making almost any kind of Philosophical Observation; yet the certainty and accurateness which is attributed to what they deliver, must be restrain'd to what they teach concerning those purely-Mathematical Disciplines, Arithmetick and Geometry, where the affections of Quantity are abstractedly consider'd: but we must not expect from Mathematicians the same accurateness when they deliver Observations concerning such things wherein 'tis not only Quantity and Figure, but Matter, and its other Affections, that must be consider'd. And yet, less must this be expected when they deliver such observations as, being made by the help of material Instruments fram'd by the hands and tools of men, cannot but in divers cases be subject to some
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if not many Imperfections upon their account. Divers of the modern Astronomers have so written of the spots and more shining parts or (as they call them) *Faculae*, that appear upon or about the surface of the Sun, as to make their Readers presume, that at least some of them are almost always to be seen there. And I am willing to think, that it was their having so often met with such Phenomena in the Sun that made them write as they did. And yet when I first apply'd my self to the contemplation of these late discoveries, though I wanted neither good Telescopes, nor a dark room to bring the species of the Sun into, yet it was not till after a great while, and a multitude of fruitless Observations made at several times, that I could detect any of these Solary spots, which have during many months at least appear'd so much seldomer than it seems they did before, that I remember a most ingenious Professor of Astronomy, excellently well furnish'd with Dioptrical Glasses, did about that time complain to me, that for I know not how long he had not been able to see the Sun spotted. And as for the *Faculae* that are written of as such ordinary Phenomena, I must profess to you, *Pyrophilus*, that a multitude of Observations made with good Telescopes at several places and times whilst the Sun was spotted, has scarce made me see above once any of the look'd for Brightnesses.

And as the nature of the Material Object wherewith the Mathematician is conversant, may thus deceive the Expectations grounded on what he delivers; so may the like happen by reason of the Imperfection of the Instruments which he must make use of in the sensible observations whereon the mixt Mathematicks (as Astronomy, Geography, Opticks, &c.) are in great part built. This is but too manifest in the disagreeing Supputations that famous Writers, as well Modern as Ancient, have given us of the circuit of the Terrestrial Globe, of the distance and
bigness;

bigness of the fix'd Stars and some of the Planets, nay, and of the height of Mountains: which Disagreement, as it may oftentimes proceed from the differing Method and unequal skill of the several Observers, so it may in divers cases be imputed to the greater or less exactness and manageableness of the Instruments employ'd by them. And on this occasion I cannot omit that sober Confession and Advertisement that I met with in the noble *Tycho*, who having laid out besides his time and industry much greater sums of money on Instruments than any man we have heard of in latter times, deserves to be listen'd to on this Theme, concerning which he has (among other things) the follow-

*Tycho Brahe lib. 2.
de Cometa An.
1577. p. 133.*

ing passage: *Facile (says he) lapsus aliquis penè insensibilis in Instrumentis etiam majoribus conficiendis subrepat, qui inter observandum aliquot scrupulorum primorum jacturam faciat; insuper si ipse situs & tractandi modus non tam absoluta norma perficiatur ut nihil prorsus desideretur, intolerabilis nec facile animadvertenda deviatio sese insinuat. Adde quod instrumenta usu & etate à prima perfectione degenerent. Nihil enim quod hominum manibus paratur ab omni mutatione undiquaque existit. Organa enim ejuscemodi nisi è solido metallo affabre elaborentur, mutationi aereæ obnoxia sunt; & si id quoque detur ut è metallica materia constent, nisi ingentia fuerint, divisiones minutissimas graduum non sufficienter exhibent, dumque hoc præstant, sua magnitudine & pondere seipsa ita aggravant, ut facile tum extra planum debitum aut figuram competentem dum circumducuntur declinent, tum etiam sua mole intractabilia redduntur. Quare magis requiritur in Instrumentis Astronomicis quæ omni vitio careant construendis, artificium pari judicio conjunctum, quam hæcenus à quamplurimis animadversum est. Id quod nos ipse usus longaque docuit Experientia non parvo labore nec mediocribus sumptibus comparata.*

Hitherto our noble Author. And as for the observations

tions made at Sea, the diligent *Fournier* advertises, that however many Sea-captains and others may brag of their Mathematical Observations made on Ship-board, yet he, upon tryal of many Instruments both at Sea and ashore, makes bold to affirm, that no Astronomer in the world can be sure to make his Observation at Sea within ten Minutes of the precise truth, no not (says he) upon the Sand it self within one Minute of it.

But instead of acquainting you with what may be drawn from the writings of our Hydrographer, to prove that his Assertion is rather modest than too bold, I shall observe, that the Observations even of skilful Mathematicians may hold so little, or disagree so much, when they pretend to give us the determinate *measures* of things, that I remember of three very eminent modern Mathematicians, who have taken upon them by their Experiments to determine the proportion betwixt Air and Water, the one makes not the weight of Water to exceed above a 150 times that of Air, the other reckons Water to be between 13 and 14 hundred times, and the third no less than 10000 times the heavier. Not to mention a modern and famous Writer or two, who have been so mistaken as to think, that the weight of the Water in comparison of the Air is I know not how much under-reckon'd even by this last (overbold) Estimate. And if I had leisure I could annex an Experiment partly statical, and relating to the weight of the Air, which though we made divers times in an hour, yet we miss'd of the like success twice as often in the same hour, without being able to know before-hand whether the Experiment would succeed within some pounds weight. But of this more perhaps elsewhere.

The Ends, *Pyrophilus*, which we have proposed to our selves in setting down the things by us deliver'd in this and the former Essay, are principally two.

And first, we desire that the Instances we have given you

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of the Contingency of Experiments may make you think your self oblig'd to try those Experiments very carefully, and more than once, upon which you mean to build considerable Superstructures either theoretical or practical, and to think it unsafe to rely too much upon single Experiments, especially when you have to deal in *Minerals*: for many to their ruine have found, that what they at first look'd upon as a happy Mineral Experiment has prov'd in the issue the most unfortunate they ever made. And I remember that the most experienc'd Mineralist I have hitherto been acquainted with, though his skill has been rather gainful then prejudicial to him, has very seriously told me, that he could quickly grow an extraordinary rich man, if he could but do constantly whatsoever he has done, not only two or three, but many times.

The other End, *Pyrophilus*, to which I had an Eye in writing the past Discourses, was, that they may serve for a kind of Apology for Sober and Experimental Writers, in case you should not always upon tryal find the Experiments or Observations by them deliver'd answer your expectations. And indeed it would prove a great discouragement to wary and considerate Naturalists from enriching the World with their Observations, if they should find, that their faithfulness in setting down what they observed is not able to protect them from blasting imputations of falshood, but that by publishing any thing for the good of others, they must expose their reputation to all the uncertainties to which any of their Experiments may chance to prove obnoxious. 'Tis true indeed, that if a Writer be wont to be fabulous or transcriptive, and to deliver things confidently by hear-say without telling his Readers when he does so, if his Experiments upon tryal succeed not, we may be allowed to impute their unsuccessfulness rather to him than to our selves or to chance, and need not think our selves obliged to have so much a greater
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care of his reputation than he had of his own, as for his sake to try more than once what he for our sakes never try'd so much as once. But if an Author that is wont to deliver things upon his own knowledge, and shews himself careful not to be deceived, and unwilling to deceive his Readers, shall deliver any thing as having try'd or seen it, which yet agrees not with our tryals of it; I think it but a piece of Equity, becoming both a Christian and a Philosopher, to think (unless we have some manifest reason to the contrary) that he set down his Experiment or Observation as he made it, though for some latent reason it does not constantly hold; and that therefore though his Experiment be not to be rely'd upon, yet his sincerity is not to be rejected. Nay, if the Author be such an one as has intentionally and really deserved well of Mankind, for my part I can be so grateful to him, as not only to forbear to distrust his Veracity, as if he had not done or seen what he says he did or saw, but to forbear to reject his Experiments, till I have tryed whether or no by some change of Circumstances they may not be brought to succeed. Thus a while since finding in Sir *Francis Bacon*, that he delivers as a somewhat unlikely truth, that Spirit of Wine will swim upon Oyl (of Almonds) we forthwith made tryal of it, but found the Oyl swim upon the Spirit of Wine, and this upon several tryals before Witnesses: but our tenderness of the reputation of so great and so candid a Philosopher made us to bethink our selves, that (though he mentions it not, nor perhaps thought of any such thing, yet) possibly he may have used Spirit of Wine more pure than ordinary; and thereupon having provided some that was well rectifi'd, we found that the Oyl that was wont to swim upon Spirit of Wine, not freed from its aqueous parts, did readily sink, and quietly lye in the bottom of that which was carefully dephlegm'd. And so having been inform'd that the learned *Dr. Brown* somewhere de-

livers, that *Aqua fortis* will quickly coagulate common Oyl, we pour'd some of those Liquors together, and let them stand for a considerable space of time in an open Vessel, without finding in the Oyl the change by him promised, (though we have more than once with another Liquor presently thickned common Oyl.) Whereupon being unwilling that so faithful and candid a Naturalist should appear fit to be distrusted, we did again make the tryal with fresh Oyl and *Aqua fortis* in a long-neck'd Vial left open at the top, which we kept both in a cool place, and after in a digesting Fornace; but after some weeks we found no other alteration in the Oyl than that it had acquir'd a high and lovely tincture: notwithstanding which being still concern'd for the reputation of a person that so well deserves a good one, the like Contingencies we have formerly met with in other Experiments, made us willing to try whether or no the unsuccessfulness we have related might not proceed from some peculiar though latent Quality, either in the *Aqua fortis* or the Oyl by us formerly employ'd; whereupon changing those Liquors, and repeating the Experiment, we found after some hours the Oyl coagulated almost into the form of a whitish Butter. Nor dare I allow my self to be confident, that I shall not need to be dealt with by you upon some occasions with the like equity that I have been careful to express towards others. And since the writing of thus much of this very Essay, having desir'd a very skilful and candid Chymist to do me the favour to provide me some of the purest and strongest Spirit of Salt that could be made; he kept some Salt in a vehement fire for divers days and nights together, and freed the extracted Liquor so carefully and so skilfully both from its phlegm and its terrestrial feces, that after all I have written in the former Essay concerning that Menstruum, I must freely confess to you, that I am now satisfi'd, that a Spirit of Sea-salt may without any unsincerity be

be so prepar'd as to dissolve the body of crude Gold, though I could not find that the Solutions I made of that Metal were red, but rather of a yellow or golden colour, much like those made with common *Aqua Regis*. But neither this Artist nor I have been since able to make another Spirit of Salt capable of dissolving Gold, notwithstanding all the industry we have employ'd about it, which makes me refer this to Contingent Experiments; unless the prosperous event of our former tryal may be ascrib'd to the quality of the Salt that was distill'd, which was brought from the Island of *Mayo*, where the scorching Sun makes out of the Sea-water a Salt that is accounted much stronger and more spiritous than that which is wont to be made in *France* and other more temperate Climats. And let me, *Pyrophilus*, take this opportunity to add, that if I had not very cautiously set down the Observation I related in another Essay * concerning the little Fishes or Worms I there teach you to discover in Vinegar, I should perhaps need much of your equity to keep me from being thought to have impos'd upon you in what I there delivered. For I have since met with divers parcels of Vinegar wherein the Observation could not be made, for one wherein it held; so that I am glad to keep by me some Vinegar stock'd with those scarce visible Animals to satisfy ingenious men, among whom some have been fain, after their own fruitless tryals, to come to me to show them the things delivered in that Observation. What I mention'd a little above to have been try'd upon Sallet-oyl, puts me in mind of telling you, that among our Experiments concerning the changes of colours, we were about to acquaint you with one which we had formerly made upon common Oyl-Olive, it seeming to us a not inconsiderable one, since it was a way that we devis'd of instantly changing the colour of the
Oyl.

* This is one of those that make up the Book of the Usefulness of *Experimental Philosophy*.

Oyl from a pale Yellow to a deep Red, with a few drops of a Liquor that was not red but almost colourless. This Experiment, as we were saying, *Pyrophilus*, we were about to set down among others concerning Colours, but because we do not willingly rely on a single tryal of such things as we know not to have been ever try'd before, we thought it not amiss for greater security to make the Experiment the second time, but could not then find it to succeed, nor even since upon a new Trial (probably by reason of some peculiar quality in that particular parcel of Liquor we first made use of) which made us think fit to omit the intended mention of it; but if I had upon my first trial acquainted you with it without any further scruple, you might upon trial have suspected, if not concluded, that I had misinform'd you, though I had really deliver'd nothing but what I had try'd. And indeed, *Pyrophilus*, though I have not the vanity to pretend to have deserv'd so much of you as such Naturalists as Sir *Francis Bacon* have deserv'd from every ingenious Reader of their Books; yet perhaps you will do me but Right to believe, that though some of the Experiments I have deliver'd may prove Contingent, yet I have not deliver'd them unfaithfully in reference to what I thought I observed in them and remembered of them. And though I desire you should so read my writings as to give no farther assent to my Opinions than the reasons or Experiments produc'd on their behalf require, yet in matters of fact which I deliver as having try'd or seen them, I am very willing you should think, that I may have had the weakness to be mistaken, but not an intention to deceive you.

There is yet one thing more that I shall venture to acquaint you with before I conclude this Essay, though you may think it relishes of a Paradox, and it is this: That when I am satisfi'd of the Abilities and Circumspection of a Writer, delivering a matter of fact as upon his own know-

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knowledge; I do not presently reject his Observation as untrue; much less condemn the Person himself as a Lye, whensoever I find that it seems to be contradicted by a contrary and more undoubted Observation, or to contradict a receiv'd and plausible either Hypothesis or Tradition: but rather try if by fit Distinction or Limitation I can reconcile them; unless I can imagine something or other which might probably lead him to mistake. And of this indulgence to an intelligent Writer I have this reason to give, That *sometimes* there happen irregularities contrary to the usual course of things, as is evident in Monsters; and *sometimes* the received Hypothesis, though perhaps not to be rejected as to the main, will not hold so universally as men presume; and *sometimes* too the contradiction betwixt the Observations may be but *seeming* (by reason of the want of some unheeded Circumstance necessary to make them inconsistent) and so they may both be true.

We might dilucidate and confirm what we have newly delivered by several Instances, were it not that this Essay is already but too prolix. Wherefore we shall only recommend to your Consideration these few Particulars.

That the Irish Spiders (of which, whatever is vulgarly believ'd to the contrary, my self have in *Ireland* seen divers) are not poisonous is not doubted by the Inhabitants, who have had many Ages experience of their harmlessness: and yet I dare not deny what the learned *Scaliger* somewhere affirms, that in (his Country if I mis-remember not) *Gascony* their venom is so pernicious, that they sometimes poison those that tread upon them through the very soles of their shoes. And that even here in *England* (though a Country so near to *Ireland*) some Spiders (at least) are venomous even without biting, I may elsewhere have occasion to give you an experimental proof.

It is so much taken for granted by divers Authors, who pretend likewise to give reasons of it, and by the generality

lity of their Readers, that under the same Meridian the magnetick Needle keeps every where the same variation without changing it by being carried Northwards or Southwards, that 'tis like if many Persons better acquainted with Magnetick Speculations than Trials, should read in the relations of the Hollanders, that under the Meridian that passes by the Island of *Corvo*, where the Needle points directly at the Poles, and which is therefore wont to be reckon'd the first Meridian, they found at two places, the one about 46, the other about 55 Degrees of Northern Latitude, a Declination in the former of those Elevations of no less than 7 or 8 Degrees, and in the latter of a far greater number; and also that they found under the 20. Parallel of Southern Latitude under the same Meridian of the *Azores* 10 or 11 Degrees of Declination; many, I say, if they should meet with these particulars, probably would suppose the Dutch to have been very bad Observers, because these Observations do not (as we intimated above) agree with the Theory of the Needles Declination. And yet if we confer these Observations with others of the like nature made by good Navigators and other skilful men along other Meridians, we may, I suppose, find cause rather to rectifie the general opinion than reject the Dutch Observations for their disagreeing with it, especially if we take into consideration what is affirmed by the Jesuite *Jules Alenis* (whom *Fournier*, amply treating of Longitudes, extols for the accuratest Observer of the Needles variation that ever was) sailing into *China* in a great *Portugal* Carraque, and accompanied by the famous Pilot *Vincent Rodrique*, who had then made 28 Voyages to the *Indies*. For out of one of this Fathers Letters *Fournier* has preserved this memorable passage,

De la Longitude
chap. xxxiv.

You must (says he) take notice of one thing very considerable, namely, that the further you go from the Æquator in the same Meridian, the greater

greater you will find the *Magnetical variation*. There are some eminent modern Naturalists who affirm, that they have assuredly try'd by Weather-glasses, that Cellars and other Subterranean places are colder in Winter than in Summer: and yet not to oppose to this Experiment the common Tradition to the contrary, I remember, that the bold and industrious Captain *James* (formerly mention'd) in the relation of his strange Voyage published by his late Majesties command, has this notable Observation, where he relates the excessive coldness of the water they met with in Summer in that Icy Region where they were forced to Winter in the Year 1632. *Moreover our Well* (says he) *out of which we had water in December, had none in July.*

Lastly, though in the Western parts it have been observed, that generally the inside or heart as they call it of Trees, is harder than the outward parts, yet an Author very well vers'd in such matters treating of the building of Ships, gives it us for a very important Advertisement touching that matter, that they have observed at *Marseilles*, and all along the Levantine shores, that that part of the Wood that is next the Bark is stronger than that which makes the heart of the Tree. But to draw at length to a conclusion of this already too tedious Essay; The Ends above mention'd, *Pyrophilus*, being those which I propos'd to my self in writing the past Discourse, you will make an use of it, which I was very far from intending you should, if you suffer it to discourage you from the vigorous prosecution of your Enquiries into Experimental Knowledge: nor indeed is any thing that hath been said fit to perswade you to other than Watchfulness in observing Experiments, and Wariness in relying on them, but not at all to such a despondency of mind as may make you forbear the prosecution of them: for neither doth

*Fourn. Archite-
cture Navale
chap. 22.*

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the Physician renounce his Profession, because divers of the Patients he strives to cure are not freed from their diseases by his Medicines, but by death; nor doth the painful Husbandman forsake his Cultivating of the ground, though sometimes an unseasonable storm or flood spoils his Harvest, and deprives him of the expected fruit of his long toils. For as in Physick and Husbandry, those that exercise them are kept from deserting their professions, by finding, that though they sometimes miss of their Ends, yet they oftentimes attain them, and are by their Successes requited not only for those Endeavours that succeed, but for those that were lost: so ought we not by the Contingencies incident to Experimental attempts to be deterr'd from making them, because not only there are many Experiments scarce ever obnoxious to casualties, but even among those whose event is not so certain, you may very probably make an Experiment very often without meeting with any of those unlucky accidents which have the power to make such Experiments miscarry; and sure the prosperous success of many succeeding attempts is well able to make amends for the fruitless pains employ'd on those few that succeed not; especially since in Experiments it not unfrequently happens, that even when we find not what we seek, we find something as well worth seeking as what we miss. Of this last-mention'd truth we may else-where have occasion to discourse more largely, and therefore shall now conclude with barely minding you, that even Merchants themselves are not wont to quit their profession, because now and then they lose a Vessel at Sea, and oft-times their Ships are by contrary winds and other accidents forc'd to put in at other Ports than those they were bound for. Which example I the rather make use of, because that as the *American* Navigators employ'd by the *European* Merchants having been by storms forc'd from their intended course, have been some-
times

times thereby driven upon unknown Coasts, and have made discovery of new Regions much more advantagious to them than the fairest and constantest winds and weather could have been; so in Philosophical Trials, those unexpected accidents that defeat our endeavours do sometimes cast us upon new discoveries, of much greater advantage than the wonted and expected success of the attempted Experiment would have proved to us.

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SOME
SPECIMENS
OF AN
ATTEMPT

To make
CHYMICAL EXPERIMENTS

Useful to Illustrate the Notions of the
CORPUSCULAR PHILOSOPHY.



LONDON,
Printed for Henry Herringman, Anno 1668.

SOME
SPECIMENS

OF AN
ATTEMPT

TO MAKE
CHEMICAL EXPERIMENTS

ON THE
CORROSION OF METALS



LONDON:
Printed for Long, Newman, and Co. 1828



THE PREFACE,

Giving an account of the two following Treatises, and Proposing the Desirableness of a good Intelligence betwixt the Corpuscularian Philosophers and the Chymists.

Here are many Learned Men, who being acquainted with Chymistry but by report, have from the Illiterateness, the Arrogance and the Impostures of too many of those that pretend skill in it, taken occasion to entertain so ill an opinion as well of the Art as of those that profess it, that they are apt to repine when they see any Person capable of succeeding in the study of solid Philosophy, addict himself to an Art they judge so much below a Philosopher, and so unserviceable to him: Nay, there are some that are troubled when they see a Man acquainted with other Learning countenance by his Example sooty Empiricks, and a study which they scarce think fit for any but such as are unfit for the rational and useful parts of Physiology. I now take notice of these things, because they gave occasion to the two following Treatises. For perceiving divers years ago, that some Learned Men of the temper above describ'd thought it strange (if not amiss also) that one of whose studies they were pleas'd to have too favourable an Expectation, should spend upon Chymical tryals (to which I then happen'd to be invited by the opportunity of some Furnaces and some leisure) much of those Endeavours

deavours which they seem'd to think might be far more usefully employ'd than upon such an empty and deceitful Study; perceiving this, I say, I thought it not amiss to endeavour to manifest, that without seeking after the Elixir that Alchymists generally hope and toyl for, (but which they that knew me knew to be not at all in my aim) I did not in the Prosecution of Chymical tryals do any thing either without an end, or unsuitable to the Design I had of attempting to promote Mens Knowledge of the works of Nature, as well as their Power over them. In order to this, I did not think it enough to shew, that by an In-

See the Essay of the Usefulness of Chymistry in the II. Sect. of the I. Part Of the Usefulness of E. P.

sight into Chymistry one may be enabl'd to make some Meliorations (I speak not of Transmutations) of Mineral and Metalline Bodies, and many excellent Medicines for the Health of Men, besides divers other Preparations of good use in particular Trades, and in several Occurrences of Humane Life; I did not, I say, think it enough to do this, because that though this might suffice to evince that a rational man might without losing his time employ some of it to understand and promote Chymistry; yet this would scarce suffice to manifest it to be useful to Philosophy. And therefore there seem'd requisite some Specimens, which might shew that Chymical Experiments might be very assistant even to the speculative Naturalist in his Contemplations and Enquiries.

But against my attempting any thing of this Nature, three Difficulties oppos'd themselves. The first was the want of Leisure, in regard I was already pre-engag'd to write of other Subjects, and to prosecute some Experiments, whose event I was concern'd to know. Another Impediment was, that for other Reasons elsewhere mention'd, and chiefly to keep my Judgment as unprepossess'd as might be with any of the Modern Theories of Philosophy, till I were provided of Experiments to help me to judge of them, I had purposely refrain'd from acquainting my self thoroughly with the intire System of either
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the Atomical, or the Cartesian, or any other whether New or Reviv'd Philosophy; and therefore I could scarce be fit to shew how Chymical Experiments might illustrate their Doctrines. And thirdly, some of those Learned Men for whom I was to write, more favouring the Epicurean, and others (though but a few) being more inclinable to the Cartesian opinions, it seem'd very difficult to gratifie by the same Compo-
sures Persons of differing Perswasions.

But as to the first of these Discouragements, since my Pre-
engagements to other Themes were not unknown to those for whom I was to write, it might reasonably be presum'd they would over-look such unaccurateness as should appear imputable to haste: And besides, some such Subject might be chosen to write of, as would conveniently admit Enlargements and Additions, according as my leisure should afterwards serve me to annex them

On occasion of the second Impediment, I remember'd, that having divers years before read the Lives of the Atomical, among other Philosophers, in Diogenes Laertius, and having sometimes occasionally heard mention made of divers Epicurean and Cartesian Notions, and having hence fram'd to my self some general, though but imperfect, Idea of the way of Philosophizing my friends esteem'd; I thought I might without a more particular and explicit Enquiry into it, say something to illustrate some Notions of it, by making choice of such as, being of the more simple and obvious, did not require skill in the more mysterious points of the Hypothesis they belong'd to.

And as for the last of the three Discouragements above mention'd, I consider'd, that the Atomical & Cartesian Hypotheses, though they differ'd in some material points from one another, yet in opposition to the Peripatetick and other vulgar Doctrines they might be look'd upon as one Philosophy: For they agree with one another, and differ from the Schools in this grand & fundamental point, that not only they take care to explicate things intelligibly; but that whereas those other Philosophers give only a general and superficial account of the Phenomena of Nature

from certain substantial Forms, which the most ingenious among themselves confess to be Incomprehensible, and certain real Qualities, which knowing men of other Perswasions think to be likewise Unintelligible; both the Cartesians and the Atomists explicate the same Phenomena by little Bodies variously figur'd and mov'd. I know that these two Sects of Modern Naturalists disagree about the Notion of Body in general, and consequently about the Possibility of a true Vacuum, as also about the Origine of Motion, the indefinite Divisibleness of Matter, and some other points of less Importance than these: But in regard that some of them seem to be rather Metaphysical than Physiological Notions, and that some others seem rather to be requisite to the Explication of the first Origine of the Universe, than of the Phenomena of it in the state wherein we now find it; in regard of these, I say, and some other Considerations, and especially for this Reason, That both parties agree in deducing all the Phenomena of Nature from Matter and local Motion; I esteem'd that notwithstanding these things wherein the Atomists and the Cartesians differ'd, they might be thought to agree in the main, and their Hypotheses might by a Person of a reconciling Disposition be look'd on as, upon the matter, one Philosophy. Which because it explicates things by Corpuscles, or minute Bodies, may (not very unfitly) be call'd Corpuscular; though I sometimes style it the Phœnician Philosophy, because some ancient Writers inform us, that not only before Epicurus and Democritus, but ev'n before Leucippus taught in Greece, a Phœnician Naturalist was wont to give an account of the Phenomena of Nature by the Motion and other Affections of the minute Particles of Matter. Which because they are obvious and very powerful in Mechanical Engines, I sometimes also term it the Mechanical Hypothesis or Philosophy.

By such considerations then, and by this occasion, I was invited to try whether without pretending to determine the above-mention'd controverted points, I could by the help of the Corpuscular Philosophy, in the sense newly given of that Appellati-
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on, associated with Chymical Experiments, explicate some particular Subjects more intelligibly than they are wont to be accounted for, either by the Schools or the Chymists. And however since the vulgar Philosophy is yet so vulgar, that it is still in great request with the Generality of Scholars; and since the Mechanical Philosophers have brought so few Experiments to verify their Assertions, and the Chymists are thought to have brought so many on the behalf of theirs, that of those that have quitted the unsatisfactory Philosophy of the Schools, the greater Number dazl'd as it were by the Experiments of Spagyrist, have imbrac'd their Doctrines instead of those they deserted; For these Reasons, I say, I hop'd I might at least do no unseasonable piece of service to the Corpuscular Philosophers, by illustrating some of their Notions with sensible Experiments, and manifesting that the things by me treated of, may be at least plausibly explicated without having recourse to inexplicable forms, real Qualities, the four Peripatetick Elements, or so much as the three Chymical Principles.

Being once resolv'd to write some such Specimina as I formerly judg'd requisite, I soon bethought my self of the Experiment hereafter deliver'd concerning Salt-Petre, divers of whose Phenomena I did also, as time would permit, cast into one of the Essays I was then engag'd to write to a Friend. And having dispatch'd that little Treatise, it found so favourable a Reception among those Learned Men into whose hands it came, that I was much encourag'd to illustrate some more of the Doctrines of the Corpuscular Philosophy, by some of the Experiments wherewith my Furnaces had suppli'd me; which also as occasion serv'd I did, partly by writing some Physico-Chymical Treatises, and partly by making such large Notes on the Essay concerning Salt-Petre, as might plentifully contribute to the History of Qualities, of which I had sometimes thoughts. And this continu'd, till in the year before the
 last, the publick Confusions in this (then un-
 happy) Kingdom reducing me to quit my former Design, toge-
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ther with the place where my Furnaces, my Books, and my other Accommodations were, I fell afterwards upon the making of Pneumatical tryals, whereof I lately ventur'd to give the Publick an account in a Book of New Experiments Physico-mechanical about the Air.

I should not trouble the Reader with so prolix a Preface to such small Treatises as those whereto this is prefix'd, but for these two Reasons. The one, that I hope the fore-going Narrative will make me be the more favourably judg'd by the Philosophers I desire to serve, if sometimes I write less skilfully of their Opinions than perhaps I should have done had I allow'd myself to search into them: And the other, that I am earnestly solicited to publish some other Tracts, tending to the same purpose that these do; to which also should I ever be induc'd, by the Reception these may meet with, to trouble the World with them, the same Preface as it is now penn'd may serve for an Introduction. I had almost forgot to take notice, That whereas at the end of the Essay concerning Salt-Petre I mention'd a then newly-publish'd Treatise of the laborious Glauber's, which I had not then perus'd, I found it to contain some Observations concerning the History of Salt-Petre, which, if they be true, are considerable enough: I must again recommend the examination of them to the Readers Curiosity, having been hinder'd by divers Avocations from saving him that labour my self. And whereas also some years after I was inform'd of another little Book he had put out since the former, wherein he teaches us a way of purifying Salt-Petre, to make a Conjunction of the spirituous and fixter parts of it, and then to suffer the Mixture to evaporate and so crystallize into Nitre; This would I confess have made me apprehensive of passing for a Plagiary with those that did not know me, but that it was easie for me to clear my self by the Testimony of very Learned Men, who had some years before perus'd my Treatise, and especially of one person, (well known by his Writings) who was pleas'd to like it so well, as to desire he might translate it, and
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had accordingly long before turn'd it into very elegant Latin. I might perhaps venture to adde, that though I could not justifie my self by so convincing a proof of my Innocence, yet he that shall take the pains to consider, that I could not borrow of Glauber the various Phenomena I have particularly set down, and much less the Reflections on them, & shall compare in what differing manners, and to what differing purposes, we two propose the making of Salt-Petre out of its own Spirit, and fixt Salt (He but prescribing as a bare Chymical Purification of Nitre, what I teach as a Philosophical Redintegration of it;) He, I say, who shall compare these things together, will perchance think, that I was as likely to find this last nam'd Experiment as another. Which things I say not, as if I scrupl'd to make use of the industrious Glauber's or any other mans Experiments, especially when I borrow not with them any of the Doctrines I build on them; but because since I neither did nor could take any notice of Glauber's Book in mine, I judg'd it requisite to say something to prevent my being thought to have unthankfully taken one of the chief Passages of my Discourse from a Book to which I was utterly a stranger.

The Reasons of my thus consenting to publish the following History of Fluidity and Firmness, without the rest of those Annotations which I writ upon the same Essay touching Salt-Petre, are partly, that these are my recentest Composures of this Nature, (having been writen but the last year save one) and were set down when I allow'd my self to be less unacquainted with Writers addicted to the modern Philosophy; partly also, because the considerableness of the Subject invited me to make these Annotations much more copious, and somewhat less unaccurate, than my Notes upon almost any other part of the Essay; & partly, (and indeed principally) because mention being sometimes made of this History in my freshly-publish'd Physico-Mechanical Experiments, both the Printer, and some Learned Gentlemen who were pleas'd to think that Book not unworthy the Translating, have sollicit'd me to let this Treatise be annex'd.

annexed to the several Versions they are about of that Pneumatical piece, and to the English Edition of the three fore-going Discourses, which the Printer fears would, without the company of these or some others, make but too thin a Book.

And I thought fit to premise to this History, the Essay concerning Salt-Petre, not only because it might appear very improper to publish Annotations without the Text it self whereunto they relate; but indeed because I find that there are still many Learned Men, of the same disposition with those I have mention'd in the beginning of this Preface; whence I am invited to divulge this Essay by the same Considerations that at first induc'd me to write it. Especially since I remember not that among the new Philosophers I have met with any one Experiment that does so fairly and sensibly accommodate so many of their Opinions. And indeed I freely confess, that I shall think my self to have done no useless service to the Commonwealth of Learning, if I prove so fortunate, as by these, or any other Writings of mine to the like purpose, to beget a good understanding betwixt the Chymists and the Mechanical Philosophers, who have hitherto been too little acquainted with one anothers Learning: There being to this very day a great and almost general Mis-understanding betwixt the Corpuscular Philosophers and the Chymists; most of Those (on the one hand) looking upon the Spagyristes as a company of meer and irrational Operators, whose Experiments may indeed be serviceable to Apothecaries, and perhaps to Physicians, but are useless to a Philosopher that aims at curing no disease but that of Ignorance; and most of the Spagyristes (on the other hand) looking upon the Corpuscularians (if I may so call them) as a sort of empty and extravagant Speculators, who pretend to explicate the great Book of Nature, without having so much as look'd upon the chiefest and the difficultest part of it, namely the Phænomena that Their Art has added to the former Edition of this vast and obscure Volume. But that some of the principal of the Hermetick Opinions may be more handsomely accom-

accommodated by the notions of the Phœnician Hypotheses, than by the common Philosophy of Elements and substantial forms, (which yet their Writers so frequently allude to and otherwise employ) may appear from hence, that whereas the Schools generally declare the transmutation of one Species into another, and particularly that of baser metals into Gold, to be against Nature, and Physically impossible; the Corpuscular Doctrine rejecting the substantial forms of the Schools, and making Bodies to differ but in the Magnitude, Figure, Motion or Rest, and Situation of their component particles, which may be almost infinitely varied, seems much more favourable to the Chymical Doctrine of the possibility of working wonderful changes, and even transmutations in mixt Bodies. And on the other side, there are scarce any Experiments that may better accommodate the Phœnician principles, than those that may be borrowed from the Laboratories of Chymists. For first, Chymistry enabling us to depurate Bodies, and in some measure to analyse them, and take asunder their Heterogeneous parts, in many Chymical Experiments we may better than in others know what manner of Bodies we employ, Art having made them more simple or uncompoundd than Nature alone is wont to present them us. And next, many Chymical operations being performed in close, and yet in transparent vessels, we may better know what concurs to the effects produced, because adventitious Bodies (or at least all grosser ones) are kept from intruding upon those whose Operations we have a mind to consider. And lastly, the Bodies employ'd by the Chymists being for the most part active ones, the progress of Nature in an Experiment, and the series of successive alterations through which the matter passes from first to last, is wont to be made more nimbly, and consequently becomes the more easie to be taken notice of and comprehended. So that all this considered, I hope it may conduce to the Advancement of Natural Philosophy, if, as I said, I be so happy as, by any endeavours of mine, to possess both Chymists
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and Corpuscularians of the advantages that may redound to each Party by the Confederacy I am mediating between them, and excite them both to enquire more into one anothers Philosophy, by manifesting, that as many Chymical Experiments may be happily explicated by Corpuscularian Notions, so many of the Corpuscularian Notions may be commodiously either illustrated or confirmed by Chymical Experiments.



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PHYSICO-CHYMICAL

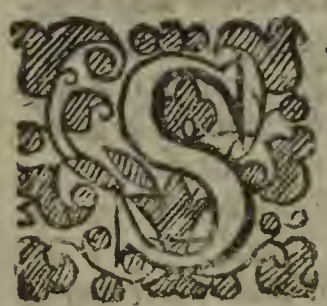
ESSAY.

CONTAINING

An Experiment with some Considerations touching the differing Parts and Redintegration of

SALT-PETRE.

SECTION I.



SALT-PETRE, *Pyrophilus*, though in that form wherein it is sold in Shops, it be no very obvious Concrete; yet either in its rudiments, or under several disguises, it is to be found in so great a number of Compound Bodies, Vegetable, Animal, and even Mineral, that it seems to us to be not only one of the most Catholick of Salts, but so considerable an Ingredient of many sublunary Concretes, that we may justly suppose it may well deserve our serious enquiries, since the knowledge of

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it may be very conducive to the discovery of the Nature of several other Bodies, and to the improvement of divers parts of Natural Philosophy.

S E C T. II.

But not having at present much leisure allow'd me by several avocations to make accurate Enquiries into the nature of Salt-Petre in general; and, which is more considerable, being not yet furnish'd with a competent number of Experiments requisite to such a purpose; I must content my self for this time to tender you some assistance towards the discovery of how differing Substances may be obtain'd from Nitre, and compound it again, by presenting you some Reflections on an Experiment, which my desire to hasten to another Subject obliges me to set down nakedly, as I first try'd it, by way of Narrative.

S E C T. III.

We took then common Nitre (as we bought it at the Druggists) and having by the usual way of Solution, Filtration, and Coagulation, reduc'd it into Crystals, we put four ounces of this purifi'd Nitre into a strong new Crucible * in which (the Vessel being first

* If it be here demanded, Why the Experiment was not made with a greater quantity of Salt-peter?

we may answer, That the mention'd Quantity was most proportionate to the best Crucible we then had. And if it be further ask'd, Whether it were not the better way of obtaining the several substances separable from Nitre, to distil it in close Vessels without addition of any foreign Body? we shall reply only by representing, That the propos'd way is not so practicable as one would imagine: for not acquiescing in the common practice of Chymists, who are wont to mingle with the Salt-Petre they distil three or four times its weight of Brick, Earth, or some other Additament, which (especially in so great a proportion) may much alter the Nature of the fix'd Salt remaining behind with it; we have had the curiosity to try more than once, whether we could distil Salt-Petre per se in glass Retorts, and found, that though to avoid giving too strong a fire, we once (at least) plac'd the Retort only in a pan-ful of Sand; yet when the heat was grown strong enough to melt the Salt, it crack'd the Retort, and did partly run out at the crack; only we obtain'd some small quantity of a Liquor, which by its sourness and operation taught us what we might have expected of the rest of the volatile part of the Nitre, in case the Vessel would have held till it had passed over into the Receiver.

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thing that might unseasonably kindle the Petre) we melted it into a limpid Liquor, and whilst it was in fusion, cast into it a small live Coal, which presently kindled it, and made it boil and hiss, and flash for a pretty while: after which we cast in another glowing Coal, which made it fulminate afresh; and after that we cast in a third and a fourth, and so continu'd the operation till the Nitre would neither fulminate nor be kindled any more: after which we continu'd to keep it in a strong fire for above a quarter of an hour, that if any volatile part should yet remain, it might be forced off.

S E C T. IV.

Then taking out the Crucible, and breaking it whilst it was hot, we took out, as carefully as we could, the remaining fix'd Nitre before it had imbib'd much of the moisture of the air, and dividing it into two equal parts, we dissolved one of those portions in as much fair water as would just suffice for the solution of it, and then drop'd on it Spirit of Salt-petre till the ebullition occasion'd by the mutual action of those contrary Liquors did perfectly cease; and forthwith Filtrating this mixture, we expos'd it in a new open Vial to the air in a window; and returning to the other portion of fix'd Nitre, which we had set apart and not dissolv'd, we drop'd on that likewise of the same Spirit till the hissing and ebullition were altogether ceas'd, and then we expos'd this mixture also in an open glass Jar to the air in the same window with the former.

S E C T. V.

The event of these Trials was, that the mixture wherein fair water was employ'd, did in a few hours fasten to the lower part of the sides of the Glass wherein it was put, some saline particles, which seem'd by their form (and partly too by their shooting about the lower parts of the Vessel) to be Salt-petre; amongst whose little Crystals nevertheless there appear'd to swim very little grains

(much smaller than Mustard-seeds) of some other kind of Salt, environ'd with a downy matter, not unlike that which is oftentimes to be observ'd in Rose-water, and several other distill'd Waters when they begin to decay. The Crystals were the next day taken out, being by that time grown somewhat greater, and more numerous, and disclos'd themselves, upon tryal, to be indeed Nitrous, as well by their manner of burning, as their shape. Concerning the latter of which, since learned Modern Writers have mis-represented it, some making Nitre to be Cylindrical, and others of a figure less approaching to the true one; I think my self oblig'd in this place to observe to you by the way, that having purposely consider'd some large Crystals of refin'd and unanalyz'd Nitre; the figure being in such best discern'd, they appear'd to have each of them six flat sides (not always of equal breadth in respect of one another) whereof any two that were opposite were commonly parallel. But to return to our augmented Crystals of Nitre: what the other matter that adher'd to them was, there was so very little of it, that we could not well discern, though we then suspected it to proceed from the want of a just or exact proportion betwixt the Volatile and fix'd parts of the Nitre that were to be re-united.

S E C T. VI.

The remaining Liquor being pour'd into an open glass Jar, and left in the same window, continued five or six days without manifesting any considerable alteration; but at the end of that time there began to appear in it very fine crystalline styriæ of Petre, which grew more and more numerous during a fortnight longer; at which time, being wearied with attending the so slow consumption of the Liquor, we pour'd it from the Crystals, and set it in a digesting Furnace to evaporate more nimbly.

S E C T. VII.

The other mixture wherein no water was employ'd did presently,

presently, for a great part of it, subside in the form of Salt; over which there swam a little liquor which also seem'd to keep the subsiding particles of Salt from congealing into one coherent mass, or so much as greater lumps: and a part of this drenched Salt being taken out and permitted to dry in the Air, did not appear very regularly figur'd, but yet seem'd here and there to recede very little from the shape of Salt-Petre, and being cast on a quick coal it burned partly after a manner not peculiar (that we have observ'd) to any distinct kind of Salt; and yet it partly seem'd to imitate the flashing way of deflagration proper to Nitre. The remaining part of this Salt, together with the Liquor swimming upon it, we kept for about a month in the open air, without discerning any observable change in the Liquor till towards the latter end of that time, and then we found it partly coagulated into small saline masses, whose figure we were not able to discern; and therefore dissolving the whole mixture in a little fair water, and filtering it, we found (after evaporation in a digesting Furnace) about one half of the Salt shot into fine small Ice-icles of the shape of Crystals of Petre, but somewhat differing from them in taste upon their first being put upon the Tongue; but upon a live coal they burned not unlike Petre. And the remaining half of this dissolution, being somewhat hastily pressed to exhale, let fall its Salt in a figure which we could not reduce either to that of Salt-Petre, or of any other determinate kind of Salt.

For the clear comprehending of this Experiment, you may be pleas'd, *Pyrophilus*, to take notice,

S E C T. VIII.

I. That a new coal is not to be cast on the Nitre till the detonation occasion'd by the former, be either quite or almost altogether ended; unless it chance that the puffing matter do blow the coal too soon out of the Crucible, (which often enough happens towards the end of the operation:)

ration :) which seems to happen chiefly because the first part of the Nitre growing to be predominant, the inflammable and halituous particles cannot break through the matter, now grown more stiff, but by such impetuous eruptions as make them oftentimes toss back the coals as soon as ever they are cast into the Crucible: and in this way of proceeding we have been forced to spend much more time, than the opinion of the ready deflagrability (if I may so speak) of Salt-Petre did beforehand permit us to imagine.

S E C T. IX.

2. That we discern'd by our Scales, that the weight of the Spirit of Nitre requisite to be drop'd on, till all the ebullition made betwixt that Liquor and the Solution of fix'd Nitre were ceas'd, did not amount to so great a weight as the Salt-Petre lost in its detonation, and yet fell not much short of it.

S E C T. X.

3. That the fix'd Nitre this way made, differ'd but very little from vulgar Salt of Tartar in its lixivate taste, in its aptness to attract the air, or to relent by the moisture of it, and in its other more obvious qualities; onely whereas Salt of Tartar is wont to be white (which nevertheless being flux'd has been by others, as us, observ'd to become of a kind of sky-colour) this fix'd Nitre was of a deep colour betwixt blew and green: which colour upon the affusion of the Spirit of Nitre vanish'd; whereas otherwise (to observe that to you upon the by) some sort of calcin'd Nitre will so obstinately retain that colour, that I keep by me a blewish green Liquor made of fix'd Petre, I know not how oftentimes successively resolv'd *per deliquium* and coagulated again, till it would no longer be reduc'd to a dry Salt, but to an unctuous body easily flowing in heat like Wax: my design in which trial it were here somewhat improper to insist on.

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That our friends might not be obliged to wait so long for the Redintegration of Nitre, but might see the Experiment made in as little time as is possible, we devis'd a more expeditious way of uniting the divorc'd parts of our Salt, and it was only by suffering such fix'd Nitre as is mention'd by our Author to run per Deliquium into a Liquor, which being separated from its faces by filtration through cap-paper was very clear and limpid: For, when we had a mind to shew the Experiment, we did onely upon this Liquor drop some Spirit of Nitre, and that, after such a noise, sparkling and effervescence, as our Author speaks of, (all which hastily vanished) did immediately associate it self with a competent proportion of the fix'd Salt swimming in the Solution, and therewith fall down in little Ice-icles of a Nitrous shape and nature; and when we pleased to continue the affusion of the acid Spirit, this emergency of Salt-Petre would be observable from time to time, till either all, or almost all the fix'd Salt had united it self with the other. And these little Ice-icles being dried in lumps, did as well upon the tongue and upon a quick coal, as they had done to the Eye, disclose themselves to be so truly Nitrous, that our friends were not wont without some wonder, as well as much pleasure, to behold Salt-Petre thus suddenly generated in lesse than a minute of an hour. These small Ice-icles being in sufficient plenty dissolv'd in fair water, we did for trials sake reduce by Congelation to fairer Crystals.

But though this be the perfectest and readiest way of reproducing Nitre, yet because it often requires, especially in dry weather, a long time to reduce fix'd Nitre per Deliquium into a Liquor, we have sometimes substituted the following way. We dissolv'd in fair water as much fix'd Nitre as we could, and filtrating the Solution through Cap-paper we satiated it with Spirit of Nitre, after the manner above describ'd, and then setting it to evaporate very slowly, and afterwards suffering it to cool, we obtain'd within some hours after the first mixture of the Liquors, store of fine little Crystals of Petre, which shot in the Liquor, the remaining part of which being evaporated afforded more of them. And though the evaporation and Crystallization cost us divers hours, yet it seem'd that the Salt-Petre was produc'd presently upon the ceasing of the conflict betwixt the two Liquors. For the mixture before evaporation tasted very like a Solution of common Nitre, and the little drops that upon the effervescence skip'd out of the Glasse, and fell back upon the sides of it, did there many of them presently coagulate into little grains of Nitrous Salt.

S E C T. XI.

And because, *Pyrophilus*, it may be suspected, that the Salt-Petre mention'd to have been produc'd by the reunion of the volatile and fix'd part of that Concrete, may have been onely some associated particles of Salt-Petre, that by lurking undiscernedly in the fix'd Nitre had escap'd the analyzing violence of the fire, and by the affusion of fair water were set at liberty to assemble together, and thereby disclose themselves in their true shape.

To remove this scruple, and to let you see that much
of

the ensuing Discourse will not need your supposing, that the Experiment of the Redintegration of Petre was accurately made, and did accordingly succeed; I must here annex, that though by divers other Chymical Experiments which I have had occasion to make with Salt-Petre, I somtimes discover'd, that now and then some undiscern'd particles of the Salt-Petre may possibly escape our diligence when we make fix'd Nitre; yet those particles are too few to amount to such Crystals of Petre, as the affusion of the acid Spirit upon the lixivate Salt are capable of affording: And that we have, to satisfy our selves farther in this particular, purposely satiated, according to the former manner, a Solution of common Pot-ashes, bought of them that are wont to sell it in Shops, (who are not so foolishly knavish as to adulterate them with Salt-petre, which is much dearer than pot-ashes) and filtrating the Solution from its copious *feces*, found after evaporation, in the remaining Liquor, within about two or three days, and sometimes much sooner, pretty store of Crystalline Salt in a Nitrous figure, which though at first it tasted somewhat corrosively, (perhaps because the proportion betwixt the Nitrous Spirit and the Pot-ashes was not duely observ'd) yet after it had a while remain'd upon the tongue, the taste of it much emulated that of Salt-Petre; and part of it being cast upon a live coal, did by its blew and halituous flame discover it self to be of the nature of that Salt. To which we may add, that we likewise tried the experiment with *Aqua fortis* and Salt of Tartar, and thereby produc'd Salt-Petre, though but in small quantity, and a long time. And those two additional Experiments I the rather mention, because many of the ensuing Reflections may be justifi'd by them, although the main Experiment made on Salt-petre alone should in divers particulars be suppos'd (for we have us'd our endeavours that it may not be found,) to have been mistaken.

The Reflections which may be made on this Experiment are more than I have either the skill or leisure to prosecute, and therefore I shall content my self to present you very succinctly with a few of those that do the most readily occur to my present thoughts.

And first, this Experiment seems to afford us an instance, by which we may discern that Motion, Figure, and Disposition of parts, and such like primary and mechanical Affections (if I may so call them) of Matter, may suffice to produce those more secondary Affections of Bodies which are wont to be called Sensible Qualities.

S E C T. XIII.

And to begin with the Tangible Qualities, as Heat and Cold; it is commonly held, that Salt-Petre is in operation a Cold Body, if not one of the coldest in the world; and accordingly Physicians and Chymists are wont to give it in Fevers to allay the inward exæstuations of the blood and humors: and that profound Naturalist the Lord *Verulam* highly commends a little of it, and did for many years himself make use of it, to condense the Spirits. But what-ever it be in inward operation, certainly to the outward sense it appears very cold: And yet the parts of this so cold Body (its Spirit and Alkali, by the latter of which Chymists are wont to mean any fix'd Salt produced by burning) put together, do immediately agitate each other with great vehemency; and did in our Experiment produce such an heat, that I could scarcely endure to hold in my hand the Vial, wherein much lesse than an ounce of each was mix'd, though but leisurely and almost by drops: as if Heat were nothing but a various and nimble motion of the minute particles of Bodies. For in our Experiment, as long as that confus'd agitation lasted, so long the heat endur'd, and with that agitation it

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encreas'd and abated ; and at length when the motion ceas'd, the heat also vanish'd.

S E C T. XIV.

Upon the mixture of the two fore-mention'd Liquors there was also produc'd a very audible sound, not unlike the hissing produc'd by the quenching of a live coal in water ; and this hissing was, as that other is wont to be, accompanied with an effervescence and boyling up of the Liquor, with store of bubbles, till it was ready to run over the Vessel. This sound seem'd to proceed from the nimble and smart percussions of the ambient air, made by the swift and irregular motions of the particles of the Liquors : And such a kind of sound, but much lowder, was produc'd by the impetuous eruptions of the halituous flames of the Salt-Petre upon the casting of a live coal upon it. What interest such a smartnesse in striking the air hath in the production of Sound, may in some measure appear by the motion of a bullet, and that of a switch or other wand, which produce no sound if they do but slowly passe through the air ; whereas if the one do smartly strike the air, and the other be shot out of a Gun, the celerity of their percussions on the air puts it into an undulating motion, which reaching the Ear, produces an audible noise even at a good distance from the body, whose swift passage causes those nimble vibrations in the air, as we may elsewhere have occasion to declare. And that in the sound observable in our Experiment, the contiguous air receives many strokes from the particles of the Liquor, seems probable, by the sudden and eager tumultuation of the parts of the Liquors : and by this, that the noise encreas'd and decay'd proportionably to the ebullition of the Liquors, and ceas'd altogether as soon as the saline particles floating in them had by their conflict tir'd themselves into quietnesse. And it is to be observ'd, that the noise ended long before the heat. To the latter of which such an intestine

intestine tumult of the parts of many bodies is sufficient, as is yet uncapable to produce a sound. As we see in Amber or good hard Wax heated by rubbing; and in many Liquors which retain a considerable degree of heat a good while after the expiration of the noise they made in boyling.

SECT. XV.

We mention'd also, that our fix'd Petre was of a blewish green colour, which upon the affusion of the acid Spirit suddenly vanish'd. That disposition of parts, whereby the light reflected to the eye, was so modify'd as to produce that colour being now alter'd. And the like change we have sometimes observ'd to be producible in fix'd Nitre, by the bare leaving it a while in the moist air. To which I must add, that in some such kind of Experiments I have observ'd the copious fumes, arising from the mixture, to make the unfill'd part of the Glasse look of a reddish colour; which is not more odd than that which we have lately had opportunity to observe in Soot, which though it be so black it self, and result from the coalition of dark exhalations, yet press'd with a strong fire, has fill'd our Receivers with fumes white enough to make them look as if they were replenish'd with milk. And we have sometimes also taken great pleasure to behold the variety of Colours which may be now and then discern'd in the sublimate, made by gradually subliming in an Urinal a mixture of equal parts of only white Sal-Armoniack and black Antimony. But to wander no longer far from our present Experiment, give me leave to inform you, that a while since attempting to make Salt of Tartar, resolv'd in a little fair water, an ingredient of Salt-Petre by the addition of good *Aqua fortis*, the union of these two Liquors produced a deep green colour, which not only was diffus'd quite through the mixture, but also appear'd to reside peculiarly in certain particles of it. For having for trial sake filtred it through Cap-paper, there remained in the Filtre

a powder of a very deep and lovely colour, but in so little quantity, that we could not attempt any Experiment upon it to make it confess its nature. But this circumstance is not to be omitted, that the Salt of Tartar that was then employ'd was extraordinary pure, having been by a peculiar art (elsewhere to be taught you) brought, without any addition, into fair-figur'd Crystals almost like lumps of white Sugar-candy. To which I must add, that the same *Aqua fortis* with a Solution of other pure Salt of Tartar, did likewise produce a colour much resembling the former, but much fainter. And it is farther to be taken notice of on this Subject, partly, that Nitre it self, although it seem to have nothing of kin to Rednesse, doth in distillation yield blood-red fumes (fondly call'd by some Chymists the blood of the *Salamander*) which fall again into a Liquor that has nothing of red in it; and partly, that the fix'd Nitre, that did before appear opacous, by a new disposition of its parts conjoyn'd with those of its reimbib'd Spirit, becomes again somewhat Diaphanous and Crystalline as it was at first.

S E C T. XVI.

Upon the mixture of these two Liquors there also obtrudes it self upon the Sense a very strong and offensive smell, proceeding from the Spirit of Petre; which perhaps occasion'd some Chymists to call a Menstruum (wherein that Nitrous spirit and smell is predominant) the *Stygian* water. But though the Nitrous Spirit have a very strong and unwelcome odour of it self, yet is it made much more offensive by being pour'd on its own fix'd Salt; for upon their conflict, the matter, being vehemently agitated, doth more copiously emit such stinking exhalations than before, and sendeth forth fumes manifestly discernable as well to the Eye as Nostrils. The odour of the fix'd Nitre is very languid; but that which it discovers being dissolv'd in a little hot water is altogether differing from

from the stink of the other, being of kin to that of other alkalizate Salts. And yet the Salt-Petre from which such differing-sented bodies spring, and which may again emerge from the coalition of them, has not been observ'd, as I remember, to have any smell at all.

S E C T. XVII.

The tastes of these two bodies are as differing as any of their other qualities: for the Spirit is exceedingly acid, and may be call'd a strong and sour *Acetum Minerale*; whereas the fixt Nitre has as strong a taste of Salt of Tartar as the Spirit has of distill'd Vinegar: and yet these two bodies, whose saviours are so pungent, and so differing, do both spring from and unite into Salt-Petre, which betrays upon the tongue no heat nor corrosivenesse at all, but coldnesse mixt with a somewhat languid relish retaining to bitternesse. And though we must not conceal from you, that in our trial the redintegrated Salt-Petre had upon its first impression upon the tongue a taste more sharp and perforating (if I may so speak) than ordinary Nitre; yet that pungency may not improbably be supposed to have proceeded from some Acid particles of the Spirit that were not yet duly incorporated with, but onely loosely adherent to, the more perfectly Nitrous parts, which afterwards discover'd it self upon the tongue. And however, the difference betwixt the taste of this new Salt, and those of the Acid and Alkalizate Salts whereof it consisted, and (unquestionably) the taste of these compared with that of the crude Petre which was dissipated into them, were sufficient to warrant this Reflection.

S E C T. XVIII.

Of the other Observables presented us by our Experiment, we must, *Pyrophilus*, content our selves to mention but a few; our haste being such that it will not permit us either to enumerate them all, or to insist long on any of them.

S E C T.

SECT. XIX.

Secondly then, the proposed Experiment seems to make it somewhat questionable, whether or no Inflammability doth strictly in all mixt bodies require a distinct Sulphureous ingredient; and whether or no in some Concretes it may not result from such a contrivance of parts, as that thereby the particles of the Concrete are dispos'd to be set a moving by the adventitious whether Fiery or Calorifick Corpuscles of another body, in such numbers, and with such celerity, as may put them into that Scheme of matter which we call Flame. How violent an heat may be produc'd upon such an account as this, may in some measure appear by an Experiment wherein our present Theme Salt-petre is the main Agent. For if into a Vial fill'd with good Spirit of Nitre you cast a piece of Iron, you may perceive that the Liquor, whose parts mov'd placidly and uniformly before, manifested no heat to the touch, meeting with pores and particles in the Iron capable of very much altering the motion of its parts, (and perhaps also that of some very subtile Intercurrent matter,) those active parts do presently begin to penetrate, sever, and scatter abroad the particles of the Iron (almost as Gunpowder doth the pieces of breaking Granadoes) with such rapidity, and in such plenty and throngs, that being themselves also put into a very swift and irregular motion (whence soever it proceeds) there is hereby produc'd a heat capable (if the quantity of the Liquor and Metall be great enough) to burn his hand that holds the Vessel, and perhaps break the Vessel (if it be not very open) all to pieces; whereas by casting into the same Spirit of Nitre little lumps of Camphire, whose particles were indispos'd to occasion the like disturbance and agitation in the Nitrous Spirit, we observ'd the agitation made of the particles of the white Gum to change it onely into a yellowish and fluid seeming Oyl.

SECT.

SECT. XX.

But not to wander any further, our own experiment informs us, that Salt-Petre (which not onely is inflammable, but burns very fiercely and violently) may be produc'd by the coalition of two bodies, which are neither of them inflammable; the one being a fix'd Salt, that to become such has already suffer'd the loss of all that the fire could deprive it of, and the other being a Spirit abounding with acid particles, which kind of Salts have been observ'd to be more apt to quench than foment fire.

SECT. XXI.

And because we may else-where, God assisting, treat more particularly of the Inflammableness of Bodies, we will now add but a few lines concerning that of Nitre, that this circumstance of it might not escape our observation, namely, that upon casting Salt-Petre on a glowing coal, or upon the casting of a glowing coal into melted Salt-Petre, the Nitre will immediately take fire and flash out into blewish and halituous flames; whereas if the same Nitre be plac'd in a Crucible, though that Crucible be by degrees made glowing hot, and do immediatly with its concave surface in innumerable places touch the particles of Nitre, yet the strange Salt will be thereby melted, but not kindled. The Reason of which Phænomenon I must not now (but may on another Occasion) spend time to enquire after.

SECT. XXII.

It may also, *Pyrophz.* deserve the Enquiry, whence it proceeds that whereas the body of Salt-Petre when committed to Distillation is oftentimes very well dry'd, and consists of Saline parts which are generally accounted to be of a very dry nature, yet the spirits of Petre forc'd by the fire into the Receiver should not, like Sal-Armoniack, and some other bodies distill'd with the like heat and vessels, adhere in the form of Sublimate to the Receiver, but fall into a liquor, which does not, for ought we have
seen

seen or heard of, either totally or in part coagulate again in the cold, as we have seen Spirit of Urine and other volatile liquors (afforded by Animal substances) often do; and as we have observ'd, though rarely, ev'n in the corrosive liquor that is wont to be call'd Butter of Antimony. And the like Enquiry may be made concerning the liquidness of the distilled Spirits of decrepitated Salt, calcin'd Vitriol, and divers other bodies, which seem to have been destitute of moisture, when committed to Distillation.

S E C T. XXIII.

But this not being precisely a Phænomenon of our Experiment, we shall not here prosecute it, (though perhaps we else where may) but rather observe to you, *Pyrophilus*, that whereas good Spirit of Nitre being left in an open vessel, is wont to smoke and waste it self in an Exhalation sensible, especially if it be excited by a little heat, not only in the Nose but to the Eye; this Fugitive Spirit when it is once re-united to its own fix'd Salt, emits no such steam, though kept a good while near a considerable fire: which Instance may somewhat assist us to make out, that the most fugitive parts of Concretes may in spite of their natural Mobility be detain'd in bodies by their Union and texture with the more sluggish parts of them, among which those lighter and more active Ingredients may be so entangled as to be restrain'd from Avolation.

S E C T. XXIV.

Another thing worth considering in our Experiment is this, that upon the dropping of the acid spirit into the Alkalizate liquor, if you place the open-mouthed glasse wherein the Experiment is perform'd betwixt the light and your eye, you may plainly discern, that the Saline particles of these liquors toss one another (or are tossed by some brisk invisible substance) to the height of divers
fingers

fingers breadth up into the air, whence most of them fall back into the Vessel like a thick shower of little drops of rain: And it were worth enquiring, whence this sparkling of the parts of these mixt liquors arises; and whether the Saline Corpuscles may be conceiv'd rapidly to move differing ways, and so, thwarting each other in their courses, and rudely justling at their Occursions, some of them are forc'd to bound or fly upwards, (almost like Ivory balls meeting each other on a Bilyard-Table.) And to assist you in this Enquiry, give me leave to inform you, that the particles thus thrown into the air appear to be most of them Saline by this Observation; that soon after the fall of the fore-mention'd showers, you shall find the sides of the glasse wherein the affusion of the Nitrous spirit has been made, all embroidered with little grains of Salt, left there by those wandring drops that fell besides the liquor.

S E C T. XXV.

And let me farther observe to you, that there seems to be a very nimble agitation in the particles of the Spirit of Nitre, by this, That upon the pouring of *Aqua fortis* (whose Active part is little else than Spirit of Nitre) upon a Solution of Salt of Tartar in fair water, in which divers small lumps of the Salt remain'd yet undissolv'd, we have observ'd the acid spirit to sever the particles of the Salt with such impetuosity, that the numberlesse little Bubbles produc'd upon their Conflict, and hastily ascending in swarms from some of the little lumps, made them emulate so many little, but rapidly rising, Springs. And to make it yet appear more probable, that there may be such crossing motions in the parts of these liquors, we observ'd, that after the two contrary Salts had by their mutual conflict tir'd each other, (or rather had been upon their occurrsions fastned to one another) there would follow no farther ebullition or skipping up and down of little drops

drops of the liquors, upon the putting in of more Spirit of Nitre, unlesse there were added likewise more of the Alkalizate liquor.

S E C T. XXVI.

And, before we passe on from this Reflection, it may not be uselesse to take notice of the difference that there may be betwixt those active parts of a body which are of differing Natures, when they are as it were Sheath'd up, or Wedg'd in amongst others in the texture of a Concrete; and the same particles, when (extricated from these Impediments) they are set at liberty to flock together, and by the exercise of their nimble motions display their proper, but formerly clogg'd activity, or acquire a Disposition to be briskly agitated by some fine interfluent matter. For though in the entire body of Salt-Petre the Ingredients it consists of, or the differing substances into which the fire dissipates it, do so mutually implicate and hinder each other, that the Concrete, whilst such, acts but very languidly; yet when the parts come to be dislocated, and the halituous and Alkalizate particles are enabled or made to disband from the Concrete, and associate themselves with those of their own nature, we see with how great an activity both the acid Spirit and the fix'd Salt are endow'd.

S E C T. XXVII.

And we may yet farther observe, that it is not barely an indefinite nimbleness of motion, and activity of the particles of Saline liquors, that enables them to perform each of their particular effects: for to the production of some of these there seems requisite, besides perhaps a Modification of their Motion, a determinate Figure of the corpuscles, answerable to that of the pores of the body by them to be dissolv'd; as Spirit of Nitre corrodes Silver, but not Gold; which neverthelesse, its particles associated with those of Sal-Armoniack, and thereby acquiring a new Figure, and perhaps a differing Motion, will readily dissolve:

dissolve: and the liquor of fix'd Nitre will for the same reason, dissolve such Sulphureous and unctuous bodies as the acid spirit will not corrode; nay, and I have carefully observ'd, that there may be liquors that will not dissolve some bodies, unlesse the motion or activity of their particles be allay'd or modify'd by the mixture of fair water, or such unactive vehicles.

S E C T. XXVIII.

Another particular which in our Experiment we may take notice of, is, the unwarinesse of those vulgar Chymists who presume confidently (and indiscriminately enough) to ascribe to each of the heterogeneous Ingredients, or (in their language) Principles of a Concrete analys'd by the fire, the virtues and properties (perhaps too in an exalted degree) of the entire body. But though this be an error of very ill consequence in reference to divers Chymical preparations of Medicines; yet having else-where discours'd purposely of it, we shall here content our selves to allege against it the instances afforded us by the Experiment under consideration: for in that we may observe, that when Salt-Petre is distill'd, the volatile liquor and fix'd Salt into which it is reduc'd by the fire, are endowed with properties exceeding different both from each other, and from those of the undissipated Concrete: for the Spirit of Nitre is (as we formerly have observ'd) a kind of *Acetum Minerale*, and possesses the Common qualities to be met with in acid spirits as such; whereas the fix'd Nitre is of an Alkalizate nature, and participates the qualities belonging generally to lixivate Salts; and Salt-petre it self is a peculiar sort of Salt, discriminated by distinct properties both from those Salts that are eminently acid, as Allum, Vitriol, Sal-gemmæ, &c. and from those that are properly Alkalizate, as Salt of Tartar and Potashes; and accordingly, we may easily observe a vast disparity in the effects and operations of these three bodies.

For several, if not all of those mineral Ones which *Aqua fortis* will by corroding dissolve, the Solution of fix'd Nitre will precipitate; and divers, if not all of those Sulphureous and unctuous bodies which the Solution of fixt Nitre will dissolve, the acid spirit of Petre will precipitate. And we have in a trice re-dissolved with the Spirit a Solution of Sublimate precipitated with the other liquor: Thus, if into a Scarlet tincture made by an Infusion of Brass in fair water, we pour a little Spirit of Nitre, the shaken liquor will in a moment change its Rednesse for a kind of Yellow, which by pouring on it a little of the Solution of fix'd Nitre, may be again graduated into a somewhat Sanguine colour, sometimes paler, and sometimes perhaps deeper than the first; whereas a Solution of Selt-petre it self pour'd on either of the former tinctures, the Red or the Yellow, has not been by us discern'd to have produc'd any sensible alteration. And whereas Salt-Petre it self is partly fix'd, and partly volatile, the acid Ingredients of it are altogether volatile, the Alkalizate fix'd. But having elsewhere occasion to speak to this subject, we shall now proceed to tell you, that

S E C T. XXIX.

It may passe for another Observable presented us by our Experiment, that it gives us occasion to enquire whether the Air doth not contribute something to the artificial production of Salt-Petre, or at least to the figuration of it according to the perfecter shape belonging to that kind of Salt: for we formerly observ'd, that the Salt which was leisurely permitted to shoot of it self in the liquor expos'd to the open air, did shoot into more fair and large Crystalline Stiria, than those that were gain'd out of the remaining part of the same liquor by a more haasty evaporation, though made but in a digesting Furnace. And we have also observ'd, that when once we pour'd *Aqua fortis* on a strong Solution of Salt of Tartar, till no further effervescence was discernable

discernable betwixt them, though the mixture by a somewhat quick heat afforded a Salt that seem'd to be very Nitrous, yet it would not be brought to shoot in so fair and conspicuously-figur'd Crystals of Petre, till it had been a good while expos'd to the open air: but whether the air its self impregnated with the promiscuous steams of most of the bodies of the terrestrial globe, (and perhaps with seminal *effluvia* from some of them) do really contribute any thing either to the Production or Figuration of Saltpetre in our Experiment, I dare not yet determine, for two chief Reasons.

SECT. XXX.

Whereof the first is, because the Figuration seems not improbably ascribable, not so much to the proper efficiency of the air, as to the conveniency which by quietnesse, and a competent vehicle to move in, was afforded to the Saline particles, to conform themselves (or be conform'd by a Concourse of Agents and Circumstances) to that figure which is most natural to them. For we have observ'd already, that the fix'd Nitre which was not dissolv'd in water, before the affusion of the acid spirit, did not shoot into the wonted form of Crystals of Petre, but remain'd a kind of Nitrous powder, the acid and Alkalizate Saline particles not having a convenient vehicle to expand themselves in; but being necessitated, for want of room, to make an unseasonable and over-hasty coalition, upon which their own weight made them subside in the figures resulting from their casual concourse, and therefore probably differing from those into which the Saline corpuscles would have been dispos'd, had they been allowed a competency of vehicle and time.

SECT. XXXI.

The other reason of my hesitancy about the use of the Air in our experiment, is, that I inconsiderately forgot to try whether part of that Liquor which shot into Crystals
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in an open-mouth'd glass expos'd to the air, would not have done the like, if it had been left quiet as long as the other was, though in a vessel accurately stopp'd.*

* Whether the Air have any great interest in the Figuration or in the Reproduction of Nitre, the Author hath since examin'd by particular Tryals; but in Vessels and by ways not to be easily describ'd in few words, and therefore the further mention of them is reserv'd for another Discourse.

but whatever the Air hath to do in this experiment, I dare invite you to believe, that it is so enrich'd with variety of steams from Terrestrial (not here to determine whether it receive not some also from Cœlestial) bodies, that the enquiring into the further uses of it (for I mean not it's known uses in Respiration, Sayling, Pneumatical Engines, &c.) may very well deserve your curiosity. To encourage which, I dare at present only tell you, that though I cannot yet pretend to much experience in this particular, yet we have known such changes (seemingly Chymical) made in some Saline Concretes, by the help chiefly of the volatilizing operations of the open air, as very few, save those that have attentively consider'd what *Helmont*, and one or two other Artists, have hinted on that subject, or have made tryals of that nature themselves, will be apt to imagine.

S E C T. XXXII.

And if upon further and exacter tryal it appears that the whole body of the Salt-Petre, after it's having been sever'd into very differing parts by distillation, may be adequately re-united into Salt-Petre equiponderant to it's first self; this Experiment will afford us a noble and (for ought we have hitherto met with) single instance to make it probable that that which is commonly called the Form of a Concrete, which gives it it's being and denomination, and from whence all it's qualities are in the vulgar Philosophy, by I know not what inexplicable wayes, supposed to flow, may be in some bodies but a Modification of the

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matter they consist of, whose parts by being so and so disposed in relation to each other, constitute such a determinate kind of body, endowed with such and such properties; whereas if the same parts were otherwise disposed, they would constitute other bodies of very differing natures from that of the Concrete whose parts they formerly were, and which may again result or be produc'd after it's dissipation and seeming destruction, by the re-union of the same component particles, associated according to their former disposition.

S E C T. XXXIII.

The Redintegration (or Reproduction) of an analyz'd body, if it can be accurately and really perform'd, may give much light to many particulars in Philosophy, and would certainly be very welcome both to the embracers of the Atomical Hypothesis, and generally to those other Modern Naturalists, who aspire to such Explications of Nature's *Phænomena* as may at least be understood: all whom I wish, that though men cannot perhaps in all things, yet at least as far as they can, they would accustom themselves to speak and think as Nature does really and sensibly appear to work; and not to acquiesce in Notions and Explications of things which, strictly examin'd, are not intelligible.

Wherefore I am about to attempt a Reproduction in Vitriol, Turpentine, and some other Concretes, in which it seems not unlikely to be performable: and perhaps you may see cause to think that the Experiment of Salt-Petre, even as we have already made and proposed it, though it be not an exact and adequate Redintegration, is yet not far from being a real one; the dissipated parts of the Concrete truly re-uniting into a body of the same nature with the former, though not altogether of the same bulk.

S E C T. XXXIV.

And yet I think it requisite to represent to you, *Pyrophilus*

Philus, that Salt-Petre is a body whose parts are not Organical, and which is not so much as very compounded; and that therefore bodies that consist of more numerous Ingredients, and much more those whose Organical parts require a much more artificial and elaborate disposition or contrivance of their component particles, cannot be safely judg'd of, by what is possible to be perform'd on a body of so simple and slight a contexture as is Salt-Petre: for we see that even wine, though no organical body, nor so much as the most compounded of inanimate Concretes, when it's spirit is, though by the gentlest distillation, drawn from it, will not, by the re-union of it's constituent Liquors, be reduc'd to it's pristine Nature; because the workmanship of Nature in the disposition of the parts was too elaborate to be imitable, or repairable by the bare and inartificial apposition of those divided parts to each other: besides that in the dissociating action, even of the gentlest fire, upon a Concrete, there does perhaps vanish, though undiscernedly, some active and fugitive particles, whose presence was requisite to contain the Concrete under such a determinate form; as we see in Wine degenerating into Vinegar, where the change seems to proceed from this, that upon the Avolation or (if I may so speak) Depression) of some subtle sulphureous spirits, whose Recess or degeneration is not to be perceiv'd by any sensible diminution of bulk in the Liquor, the remaining parts fall into new leagues or dispositions, and constitute an acid Liquor somewhat fix'd and Corrosive, and consequently of qualities very differing from those of the Wine, whose souring produc'd it: as we more fully declare in our Experiments relating to Fermentation.

S E C T. XXXV.

And certainly there is, as we formerly said, so artificial a contrivance of particles requisite to the constitution
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of the Organical parts of living bodies, that it will be scarce possible for humane Art or Industry to imitate so as to equal those exquisite productions of Nature: And therefore I wonder not that the story of the *Phœnix's* resurrection out of her own ashes should by the best Naturalists be thought a meer fiction. And if that relation, mention'd by the inquisitive *Kircherus* as an eye-witness of the Reproduction (if I may so call it) of Shell-fishes near the brink of a Lake in the *Sicilian* Promontorie *Peloro*, by the watering of their broken bodies with Salt water in the Spring, be strictly true, it seems much more improbable that such changes and vicissitudes should be bare Redintegrations of the dissociated parts of such restored bodies; than that*, (according to what we elsewhere teach,) they should be New Productions made by some seminal particles undiscernedly lurking in some part of the destroyed body, and afterwards excited and assisted by a Genial and cherishing heat so to act upon the fit and obsequious matter wherein 'twas harbor'd, as to organize and fashion that disposed matter according to the exigencies of it's own Nature. For that in some bodies the Seminal particles may a while survive the seeming destruction of life, is not altogether without example, as we elsewhere professedly manifest. And in *Kircher's* story it is to be observ'd, that the restor'd Animals were but Shell-fish; in whose slimy and viscous substance the Spirits and Prolifick parts are probably both more diffused and kept from being easily dissipable; to which I know not whether it will be worth while to subjoyn, that in such Fishes the Mechanical contrivance is but very plain, and as it were slight and obvious, in comparison of the exquisitely elaborated parts of more perfect Animals.

Lib. 3. de Arte
Magnet. Par. 5.
Cap. 3.

* In some Papers
about the origine
of Living Crea-
tures suppos'd to
spring of them-
selves.

SECT. XXXVI.

The last observable, *Pyrophilus*, that we shall at present take notice of in our Experiment; shall be this, That it may thereby seem probable that some Chymical remedies may be too rashly rejected by Physicians, because Oyl or Spirit of Vitriol, *Aqua Regis*, or other Corrosive Liquors, have been employ'd in their preparation: For it is confidently affirm'd by many Physicians, and but faintly denied by some Chymists, that the Corrosive Menstruums made use of in the preparation of remedies, can never be so exquisitely wash'd off from them, but that some of the Salts will adhere to the Medicines, and perniciously display their Corrosive Nature in the body of him that takes them. And it is not to be denied, but that many ignorant and venturous Chymists do unskilfully (and therefore dangerously enough) employ Corrosives sometimes without any necessity or real advantage to invite them to it, and sometimes without sufficiently freeing their Medicines from the corroding Salts, by whose assistance they were prepar'd; for 'tis not always the frequency of ablutions, though with warm water, that will suffice to carry off the Salts from some bodies; and therefore those great Artists, *Helmont* and *Paracelsus*, prescribe *some* things to be dulcifi'd by the abstraction of the water of whites of Egges, (which though it seem insipid, hath been found a great disarmer of corrosive Salts) and *others* by the frequent distillation of Spirit of Wine, which indeed (not to mention the Balsamick parts it may leave behind) we have observ'd to have a faculty of carrying up with it the Saline Particles of Spirit of Vinegar adhering to some Chymical remedies. But all this notwithstanding, *Pyrophilus*, there may be several bodies (and perhaps more than are commonly taken notice of) which quite alter the nature of the acid Salts employ'd to prepare them, by occasioning those Salts to degenerate into another nature, upon the very act of corroding, or else
by

by so associating their own Salts with those of the dissolving Menstruum, that from the Coalition of both, there emerges a third body differing in qualities from either. As in our experiment we find that the Spirit of Petre, which is much more sharp and corrosive than the strongest distill'd Vinegar, and the fix'd Nitre which is Caustick like Salt of Tartar, and may, I suppose, well serve for a Potential Caution, (as Surgeons speak) do by their mutual action work themselves into Salt-Petre, which is far enough from having any eminently fretting Quality, and may be safely taken inwardly in a much greater Dose than either of its Ingredients.

S E C T. XXXVII.

How much corrosive Salts may dulcifie themselves by corroding some bodies, you may easily try by pouring distill'd Vinegar or moderate Spirit of Vitriol upon a competent proportion of Corals, or Crabs Eyes, or Pearls, (or, as I suppose, almost any testaceous body.) And for my part, though I am very shy of imploying corrosive Liquors in the Preparation of Medicines; yet I have lately given a Preparation of refin'd Silver made with *Aqua fortis* it self, or Spirit of Nitre, not onely innocently, but with such success, that a couple of Experienc'd Physicians themselves, that were troubled with a superfluity of serous humors, sent to request it of me for their own use.

S E C T. XXXVIII.

It were therefore worth while, in every Preparation where Corrosive Liquors are wont to be imploy'd, or may seem requisite to be so, to consider the distinct nature of the particular bodies to be wrought upon, or consult Experience *whether* or no the acid Menstruum do communicate to the Concrete any Particles capable of retaining their fretting Quality after the end of the Operation; or whether or no the Salts do not so spend and tire themselves

in the act of corroding, that being as it were sheath'd, they become unable to corrode any further; or whether or no the Menstruum do not in the body to be corroded meet with some such Saline particles, as may with it constitute a new and inoffensive substance, as when Spirit of Vinegar, by corroding calcin'd Lead, is turn'd with it into a Salt, not of an acid, but a Saccharine taste, such as invited Chymists to give it the name of Sugar of *Saturn*. In the former of these cases the Medicine may be dangerous, unless it be after the Solution or Corrosion ended exquisitely dulcifi'd from all remainder of the Corrosive Salts. But in the two latter cases the remedies may in spite of the Corrosiveness of the Menstruums employ'd about them, be safe and innocent enough; for it matters not much how sharp and fretting the sever'd Ingredients of a remedy were, provided the remedy it self resulting from them be not so. And whereas it is objected, that in divers of these remedies the Corrosive Salts are not really destroy'd, but onely disguis'd, because by distillation it is possible to separate from them the Liquors us'd about them as Corrosive as ever: it may easily be reply'd, that besides that in several Medicines, the Matter of Fact will not hold in divers others; the Objection built on it is much more specious than solid, for it very little concerns us to be sure that out of the Medicines we take or give the violence of fire cannot separate Corrosive Salts; provided we be duely satisfi'd, that no such separation can be made by the heat or Juices of a humane body. And therefore, though it have been affirmed, that *Tartarum Vitriolatum* would upon the urgent sollicitation of a strong fire, part with much of (that most fretting liquor upon animal substances) the Oil of Vitriol, that concurr'd to its Production; yet our best and wariest Physicians, not onely Chymists but Methodists, scruple not to give it inwardly in several constitutions and distempers. And to end this discourse with the Experiment that
began

began it, we clearly see that Salt-Petre is frequently and innociously given inwardly, though the Salt, that makes even *Aqua fortis* so Corrosive, be the principal Ingredient of it, and may by distillation be driven from it.

S E C T. XXXIX.

It would not have been very uneasie for me, *Pyrophilus*, to have added to divers particulars of the past Discourse, Experiments and considerations tending to countenance or illustrate the Reflections therein set down: but in the first place, I wanted leisure to expatiate; in the second place, I was unwilling to anticipate what I have to say to you in other Essays, especially expecting to have elsewhere occasion to make mention of Salt-Petre. And besides all this, I am (to tell you the truth) desirous to impose on you a kind of necessity of prosecuting this Experiment, further than when I made it I had opportunity to do. For as I am apt to think it may prove a noble one: so I am sufficiently sensible of my having not yet been able to look into the bottom of it; and that very sense of my own ignorance, help'd to keep me from lengthening your trouble in this Essay, lest by solemnly endeavouring to countenance my Conjectures, I might be thought Dogmatical in a hasty Scribe, wherein 'tis much more my design to awaken and engage your Curiosity, than acquaint you with my opinions. And yet I thought it not amiss to mention the past considerations, such as they are, partly because this one instance seems so fairly to accommodate more than one Notion of the intelligible Philosophy, which seems hitherto not to have so much as employ'd, much less produc'd, any store of Experiments; and partly, because I would have you take notice, that more Observables than one may sometimes be very reasonably sought for in a single Experiment. And perhaps too, I was willing by my spending a whole Essay upon one Experiment, without allowing my self to wander
often

often from it, to invite you to think with me, That Experiments ought to be estimated by their value, not their number; and that a single Experiment, I say not such as that the last Essay treats of, but in general, such as, it may be, may as well deserve an entire Treatise, as a great many less considerable ones. As one of those large and orient Pearls that are fit to adorn a Monarchs Crown, may outvalue a very great number of those little (though true) Pearls that are to be bought by the ounce in Goldsmiths and Apothecaries shops.

S E C T. XL.

Having newly met, *Pyrophilus*, with some small Treatises freshly publish'd by *Glauberus*, and not having now the leisure to consider, or indeed so much as to peruse, much less the opportunity to make tryal of divers Particulars, which by turning over the leaves of the book, I find mention'd by him in relation to Salt-Petre, I must recommend to you the care of examining the Particulars he delivers; and trying how far some of them may serve to correct, or to confirm, and how far others may be corrected by what has been in the past discourse set down concerning Salt-Petre upon Experiments, some of whose fruits I can yet shew you, which were made upon the account of the divisibleness of Nitre into fix'd and volatile parts, long before the Publication of *Glauber's* Treatises.

THE

THE
HISTORY
OF
FLUIDITY
AND
FIRMNESSE.

Begun by R. B.

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ADVERTISEMENTS

Concerning the following

T R E A T I S E.



HAT the Author in those Animadversions upon the Essay touching Salt-Petre, whereof the ensuing Treatise makes a part, might with the more freedom and conveniency adde, alter, and ev'n retract as he should see cause, he thought fit to write them as if they were made on the Work of another.

The Author hopes, That the Equitable Reader considering that the following Particulars touching Fluidity and Firmness were first written but by way of Annotations upon the beginning of the above-mentioned Essay, will excuse the unaccuracy of the Method, as a fault scarce evitable on that occasion. It is also hop'd, that if the Reader will remember, that he was told in the Preface to the newly-mention'd Essay, that most of those whom to gratifie, That Treatise and the ensuing Notes on it were written, were addicted to the Epicurean Philosophy; The Authors Explicating things chiefly according to the Atomical Principles will not be thought strange, nor be lookt upon as a sure Argument of his being wedded to the particular opinions wherein the Atomists differ from other modern Naturalists; especially, since he has on some occasions plainly enough

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intimated the contrary, by proposing, together with the Atomical ways of resolving a thing, another Explication more agreeable to the Cartesian, or some other modern Hypothesis.

The following Tract was entitl'd a History of Fluidity and Firmness, because indeed the having set down Experiments and other matters of fact relating to the Subjects treated of, is the Main, though not the Only thing the Author dares pretend to have done in it. And he stiles the History as it now comes abroad, Begun: Partly, because he would invite abler Pens to contribute their Observations towards the compleating of what he is sensible he has but begun; and partly because he may hereafter, if God permit, do something of that kind himself.

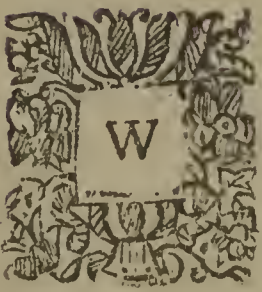
And lastly, the Author, though troubl'd that he can do it, dares not but Advertise the Reader, That some Pages, partly a little after the beginning, and partly about the middle of the following Treatise, having been lost through the negligence or mistake of him to whose Care the sheets whereon it was written were committed; he fears he has not been able, otherwise than very lamely and imperfectly, to repair that loss out of his Memory.



THE
HISTORY
OF
Fluidity and Firmness.

The First Part.
OF FLUIDITY.

SECT. I.

 W Hether Philosophers might not have done better in making Fluidity and Firmness rather States than Qualities of Bodies, we will not now examine. But under which soever of the two Notions we look upon them, 'tis manifest enough, that they are to be reckon'd amongst the most general Affections of the Conventions or Associations of several particles of matter into Bodies of any certain denomination, there being scarce any distinct portion of matter in the World that is not either Fluid, or else Stable

Stable or Consistent. And therefore, I presume, it may be well worth while to consider what may be the general causes of these two States, Qualities, or Affections of Matter; and to Try whether by associating Chymical Experiments to Philosophical Notions, there may not be given at least a more Intelligible and more Practical of both these Subjects, than has been hitherto afforded us by the Doctrine of the Schools, which is wont to appear very unsatisfactory to discerning Men; many of whom look upon what is wont to be taught by the Peripateticks, concerning Fluidity and Firmness, as well as other Qualities, to be partly too general to teach us much, and partly too obscure to be understood. And that which at present invites us to this Enquiry is, chiefly, that some Circumstances of our Authors Experiment, touching Salt-Petre, may afford us some useful assistance in our designed search. For though the chief Phænomena and Circumstances of the Experiment may be thought principally to respect Fluidity; yet since that and Firmness are contrary Qualities, and since it is truly, as well as commonly, said, That contraries survey'd together serve to illustrate each other, it may reasonably be hop'd, That the Light which the circumstances just now related to may give to the Nature of Fluidity, may facilitate the knowledge of that of Compactness: nevertheless, we shall often be oblig'd to treat of these two qualities together, because the Experiments we are to produce do many of them relate to both.

S E C T. II.

A Body then seems to be Fluid, chiefly upon this account, That it consists of Corpuscles that touching one another in some parts only of their Surfaces (and so being in-contiguous in the rest) and separately Agitated to and fro, can by reason of the numerous pores or spaces necessarily left betwixt their in-contiguous parts, easily glide along each others superficies, and by reason of their motion dif-
fuse

fuse themselves, till they meet with some hard or resisting Body; to whose internal surface, by virtue of that Motion, their Smallness, and either their Gravity, or something Analogous or Equivalent to it, they exquisitely, as to sense, accommodate themselves.

S E C T. III.

What notion *Epicurus*, and the Antient Atomists his followers, had of fluid Bodies, may be learn'd from these Verses of his Paraphrast *Lucretius* :

*Illæ autem debent ex levibus atque rotundis
Esse magis, fluido quæ corpore liquida constant.
Nec retinentur enim inter se glomeramina quæque,
Et procurfus item in proclive volubilis extat.*

And indeed, it is probable enough that in divers Liquors the little surfaces of the component particles are smooth and slippery, and that their being so does much facilitate the gliding of the Corpuscles among themselves; and consequently, the Fluidity of the Body they compose. Nor is it to be deny'd, that the Spherical figure of such Corpuscles may also conduce to their easie rouling upon one another: but there are divers other figures which may make the little Bodies indow'd with them voluble enough to constitute a fluid substance. And the other qualities to be met with in divers liquid substances, and even in water it self, and Oyl, seem to argue their parts to be otherwise shap'd, and those fluid Bodies which are not Liquors, as Air and Fire, seem to be compos'd of particles not all or most of them round, but of very various, and sometimes of very irregular figures, and yet that such Bodies deserve to be call'd fluid ones, will be manifest anon: And that they make a much more considerable part of the Universe than those that are wont to be call'd Liquors, may be argu'd from hence, that except the Earth, the Planets, and per-
haps

haps too the fixt Stars, the rest of the World, as vast as it is, seems to consist chiefly, if not only, of an Ætherial, thin, and fluid substance, as may appear (to omit other arguments) by what latter Astronomers have observ'd concerning the free and unresisted motion of such Comets as have by a Trajection through the Æther, for a long time wander'd through the Celestial or Interstellar part of the Universe.

S E C T. IV.

And here let us observe, that 'tis not necessary to the Fluidity of a Body, nay, nor to its appearing fluid to the Eye it self, That the Corpuscles it consists of be crowded as close together as they are wont to be in water, and other bodies that are commonly lookt upon as the only Liquors. For though a parcel of matter no bigger than a grain of Corn, being rarify'd into smoak, will possess an incomparably greater space than it did before; and though, if a Body be further rarify'd into flame, its expansion will be yet much greater: yet both smoak and flame may be so order'd, as to appear like Liquors. We have practis'd divers ways, to make the fumes of Bodies acquire a visibly-level superficies like water; but the easiest, though not perhaps the best, is this, (part of which I remember I have seen perform'd as a kind of trick by a very ingenious Person.) The mouth being fill'd with the smoak of Rosemary (that happening to be at hand when I made the Experiment) if this smoak be plentifully blown into a glass Pipe of an indifferent size, and open at both ends; and if when 'tis well fill'd with smoak, the lower end be presently stopt, and the glass be kept still a while in an erected posture, the fumes will settle by degrees to a level superficies like water: so that, though you gently incline the Pipe any way, the upper surface of the smoak will nevertheless quickly grow parallel to the Horizon. And if the glasse be further (but slowly) made to stoop, the smoak will seem to run down in a Body like water, whilst it continues

in the Pipe, though when it is come to the lower end of it, instead of dropping down like water, it will commonly rather flye upwards and disperse it self into the Aire. And as for flame, I fore-see I shall ere long have occasion to mention an Experiment, whereby I have sometimes endeavour'd to shew, that ev'n two contiguous flames, as expanded Bodies as they are, and as open as their Texture is, may like visible Fluids of a differing kind retain distinct surfaces.

S E C T. V.

But instead of Examining any further, how many Bodies are or may be made visibly to appear fluid ones; let us now resume the Consideration of what it is that make Bodies fluid; especially, since having intimated some of the Reasons, why we are unwilling to Confine our selves to the Epicurean notion, we hope it will the lesse be dislik'd, that we thought fit to make such a description of a fluid substance, as may intimate, that we conceive the conditions of it to be Chiefly these Three.

The first is the Littleness of the Bodies that compose it: For in big parcels of matter, besides the greater inequalities or roughnesses that are usual upon their surfaces, and may hinder the easie sliding of those Bodies along one another; and besides that diverse other Affections of a fluid Body cannot well belong to an aggregate of grosse Lumps of matter; besides these things, I say, the bulk it self is apt to make them so heavy, that they cannot be agitated by the power of those causes (whatever they be) that make the minute parts of fluid Bodies move so freely up and down among themselves: whereas it would scarce be believ'd, how much the smallness of parts may facilitate their being easily put into motion, and kept in it, if we were not able to confirme it by Chymical Experiments. But we see that Lead, Quick-silver, and ev'n Gold it self, though whilst they are of a sensible bulk, they will readily
sink

sink to the bottom of *Aqua Regis*, or any other such Liquor; yet when the *Menstruum* has corroded them, or fretted them asunder into very minute parts, those minute Corpuscles grow then so much more capable of agitation than before, that quitting the bottom of the Liquor, they are carri'd freely every way, and to the top, with the associated parts of the Liquor, without falling back again to the bottom. Nay, we see that ponderous and mineral Bodies divided into corpuscles small enough, may be made so light and voluble, as to become Ingredients ev'n of distill'd Liquors; as we may learn by what some Chymists call the Butter, others (simply) the Oyle, and others the *Oleum Glaciale* of Antimony, which though it be after Rectification a very limpid Liquor, yet does in great part consist of the very Body of the Antimony, as may appear (not to mention its weight) by this, that 'tis most easie to precipitate out of it with fair water store of a ponderous white calx, reducible by Art to an Antimonial glasse. Nay, we make a *Menstruum*, with which we can easily at the first or second Distillation bring over Gold enough to make the distill'd Liquor appear and continue ennobled with a Golden Colour.

And to show yet more particularly, that great Bodies are too unwieldy to constitute fluid ones; We may further observe, how as well Nature as Art, when either of them makes Bodies of considerable bulk fluid, is wont in order thereunto, to make a Comminution of them, as we may observe in divers Examples.

S E C T. VI.

Thus we see that in the stomachs of Dogs, Nature, to reduce Bones into those fluid Bodies, Chyle and Blood, does by some powerful and appropriated juice, (whether belonging to the Stomack it self, or thrown out of the Arteries in the passage of the circulating Blood) dissolve them into parts so minute, that the acutest Eye would not tempt

tempt a man to suspect, that such a Liquor had ever been a Bone. And that it may not be objected that this dissolution is chiefly performed, or at least must always be assisted by the Liquor which Animals take into their Stomachs by drinking; I shall represent not only that we find by experience how little, common water the only usual drink of Dogs, Wolves, &c. is able to dissolve bones though they be very long not macerated but boil'd in it; but that (if we may believe Natural Historians and credible Travellers) there are some sorts of Animals, as particularly Camels, that may be brought not to drink once in many days, ev'n when they travel in hot Climates. And to make you think this the less improbable, I shall adde, that I am familiarly acquainted with an Ingenious Gentleman, who, as himself and an ancient Virtuoso, in whose house he lives, have inform'd me, does usually drink but once in several days, and then no excessive draught neither. And when I askt him how long he had actually abstain'd not barely from drink, but from thirsting after it? He answer'd, that he had once (some few years before) continued about nine days without either taking or needing any drink; and he doubted not but that he might have continued much longer in that state, if by distempering himself one night with long and hard study he had not had some light inclination to take a small draught, which serv'd him for about four days longer. And when I askt him whether in that hot Summers day that preceded the evening wherein he happen'd to tell me this, he had not drunk at all? he answer'd Negatively. And it adds to the strangeness of this Peculiarity, that this Gentleman is in the flower of his Youth, being but about twenty two years of Age, and of a Sanguine and Florid Complexion. And (to annex that also upon the By) I learned by enquiry from him, that he sweats freely enough, as I remember I saw him do, that his Diet is the same with other mens, without re-

Z straining

straining him from the free use of Salt Meats, and that his Urine is in Quantity much like that of ordinary Men of his Age and temperament. But to return to what I was saying more generally of the Stomachical *Menstruum* of Animals; I shall adde on this occasion, that to make some kind of Imitation of it, I prepar'd, and do elsewhere mention and teach a certain Liquor, that I use, whereby I have in a short time, and without fire, dissociated the parts of roasted or boil'd flesh, bread, fruit, &c. and pull'd them asunder into very minute Bodies, whereby I have reduc'd sometimes one, sometimes another of them, together with the *Menstruum*, (which needs not much exceed them in Bulk) to the consistence of a fluid Body.

We see likewise, that Fusion makes metalls fluid, and in Fusion there is manifestly a comminution of the melted Body, the Heat alone of Gold, Silver, or Iron, though encreas'd even to Ignition, being not able to make those metalls become fluid, whilst they continue in masses of any sensible bulk. To which I shall adde anon, that even melted Metalls may have their Fluidity encreas'd by a yet further Comminution of their parts.

S E C T. VII.

And to resume here the Consideration of that very difficult Question, which we have elsewhere mention'd, it seems well worth Enquiry, whence it happens that in the distillation of common Salt and other saline Bodies, which not only are not fluid, but are hard ev'n to brittleness, there will yet be obtain'd a perfect and permanent Liquor, and from some of them a very considerable proportion of it. In answer to this Question it may indeed be said, That in diverse dry Bodies, such as Harts-horn, Wood and Bones, committed to distillation, the fire does no more than separate the aqueous or other liquid parts from the others, wherewith they were blended in the Concrete, and bring them together into the Receiver, where they
convene

convene into a Liquor. But besides that this it self is perhaps more easily said than prov'd, it does not reach the propounded Difficulty: For with what probability can it be affirm'd of Bodies that have been already calcin'd or melted? such as are the red Calx of Vitriol, and flux'd Sea Salt, &c. which yet afford Liquors, though their aqueous and other looser parts have been already driven away by a strong fire before their being expos'd to distillation. I have sometimes then consider'd, whether it may not seem less improbable to conjecture, that the vehement agitation produc'd in such Bodies by the violence of Heat, does both divide them into minute Corpuscles, and drive over swarms of them into the cold Receiver, where loosing their former vehemence of agitation, they are reduc'd into a Liquor, chiefly, (for I would not exclude concurrent causes) by reason that the fire happen'd to rend the Concrete into parts, by their extreme littleness, or their shape, or both, so easie to be tumbled up and down, that the wonted agitation of the Air, propagated by the interpos'd Bodies or Medium; or else that the same cause, whatever it be, that gives the Air its wonted agitation, is able to give such minute Corpuscles enough of it to keep them fluid.

S E C T. VIII.

That there is constantly in the Air a various motion of the small parts, will be anon declared. That also some Bodies will be kept fluid by a much less measure of agitation than is requisite to others, seems probable from hence, That Wine will continue a Liquor in such a languid warmth of the Air, as will not keep the parts of water moving, but permit them to rest in the form of Ice. And in cold Countries, where Wine it self would congeal, (as I have by Art made it do here in *England*) 'tis observ'd, that though the more aqueous parts will by the loss of their motion be turn'd into Ice, yet the more subtil and

spirituous parts remain unfrozen; and so do diverse other Liquors, (especially Chymical) of very subtil and volatile parts. And the Corpuscles that chiefly compose that Body which is properly call'd the Air, though it appears by weather-glasses that Cold may very much contribute to condense it, (that is, to occasion the approach of its parts to one another, or, reduce them to a closer order) have not been observ'd to be frozen by any degree of cold whatsoever; which seems to proceed from hence, that by reason of their extream littleness, (not excluding their figure) there cannot be so little of agitation about the Earth, as not to be sufficient to continue a various motion in such very minute Bodies, and consequently to keep them fluid.

Now, That likewise it is possible that a saline Spirit should consist at least in great part of very minute grains of Salt, we elsewhere declare, where 'tis taught, that a Sal-Armoniack may be made by Spirit of Urine and Spirit of Salt, as the common Sal-Armoniack is made with crude Salt: and there a way is also shewn, how these two Salts, (the Urinous and the other) as strictly as they are united in the compound, may be readily divorc'd. And agreeably to this I observe, that as (according to what I elsewhere note) a common *Aqua fortis* may be enabled to dissolve Gold, (on which of it self it will not fasten) by the addition of Spirit of Salt; so I find that common crude Salt barely dissolv'd in it, will give it the like power of working upon Gold. Nay, I have try'd that crude Nitre dissolv'd in good Spirit of Salt, may make it serve for an *Aqua Regis*. And I remember on this occasion, that having enquir'd of the most noted Person in *Holland* for the distilling of corrosive waters, what was the greatest proportion of distill'd Liquors that ever he was able to obtain from Sea Salt; he (though a man not given so much as to boasting) affirmed to me, that by using instead of the ordinary

nary *Caput mortuum*, as Brick-dust, Sand, &c. (that Chymists are wont to mingle with Salt before they distil it) a certain whitish clay, he had sometimes brought over almost the whole body of Salt into a Liquor; insomuch that from a pound of Salt he could draw, and that without any extraordinary trouble or degree of fire, fourteen Ounces of Liquor. And when, because I suspected that much of this might be water forc'd from the clay mingled with the Salt, I enquired whether he had ever dephlegmed this Liquor; He answered me, that he had purposely done it; and sometimes found no less than about twelve ounces of it to be strong rectify'd Spirit: which brought into my mind that almost incredible passage of *Beguinus*, who somewhere teaching the Distillation of another Salt, addes to the end of his Directions, That if you have wrought well, you shall get from a pound of the matter a pound of Spirit. But because from all these Liquors distill'd from such kind of Salts, 'tis possible either by Rectification or some more Philosophical way to obtain a portion of phlegm or water, I leave it to further Enquiry, whether or no the Fluidity of these distill'd Liquors may not in diverse cases be in part further'd by the mixture of some particles of an aqueous nature, (such being fit to make Dissolvers and vehicles for Salts) which may not absurdly be suspected to have been produc'd by the action of the fire upon the Concrete committed to distillation; if we allow with that famous Chymist *Helmont*, That by the Alchahest all gross Bodies may be totally, and that without it, ev'n Oyle and Salt may in great part, (and that without Additaments) be reduc'd into insipid water.

S E C T. IX.

We shall anon, (when we come to treat of Firmness) mention our having made a certain substance so dispos'd to Fluidity, that it may be made to change the stable consistence for a liquid one, by so small an Agitation, as only the

the Surplusage of that which the ambient Air is wont to have about the middle even of a Winters day, above what it hath in the first or latter part of it. Nay, we have made ev'n a Metalline Salt or Vitriol, capable of this proclivity to liquefaction, of which we have unquestionable witnesses. And therefore, it need not appear incredible, that other heaps or aggregates of Corpuscles much lighter than these, though heavier than those of the Air, may have all their parts so minute and fitted for motion, that the wonted agitation of the Air may not only about noon, but at all other times of the day keep them in motion, and thereby in the state of Fluidity.

S E C T. X.

And here I must adde, that 'twas not altogether without cause that I lately took notice of the shapes as well as the sizes of Bodies, in reference to their fitness to constitute fluid ones. For though I be not sure but that in those Bodies, as Sal-Armoniack, Antimony, &c. which are by the fire sublim'd into flowers rather than distill'd into Liquors, the magnitude of the component Corpuscles may not be a hinderance to the Fluidity of the Body they constitute: yet this seems as probably referable to their figure, unapt for the requisite motion, as to their bulk. And I have sometimes made to this purpose this Experiment. That by slowly distilling Oyl-Olive *per se* in a glass Retort, (plac'd in Sand) I found, as I expected, that about the third part of the Oyle, which was driven over into the Receiver, did there coagulate into a whitish Body almost like Butter. So that although it seem'd manifest by the strong smell and very piercing taste of this white substance, that the Oyle which afforded it had its particles, as it were, torn in pieces; and though distillation be wont to obtain Liquors ev'n from consistent Bodies; yet in our Experiment of a concrete that is naturally fluid, the distill'd Liquor it self proves not to be so: of which no
cause

cause seems more obvious, than that the newly-acquired shape of the dissipated parts of the Oily Corpuscles makes them unfit for motion; either Absolutely speaking, or at least in Respect of one another, by making them less pliant than formerly, or giving them a figure more easie to be entangl'd with the neighbouring Corpuscles, or else by making their surfaces less smooth and slippery than before.

S E C T. XI.

But to return thither whence we have digress'd, and mention some more familiar Examples of the Conduciveness of the smallness of a Bodies disjoyned parts to its Fluidity, we may take notice, that of Bodies that consist of incoherent parts, and are made up, as it were by Aggregation, those *de cæteris partibus*, in their being pour'd out most resemble Liquors, that are the smallest; as would appear upon the emptying of several Sacks, the one of Apples, the other of Walnuts, the third of Filberts, the fourth of Corn, the fifth of Sand, and the sixth of Flowre.

Confectioners also, Cooks, and others that make much use of whites of Eggs, will easily reduce those clammy and viscous Bodies into a thin and fluid substance, to which for its affinity with water many give the same name: and yet this difference of Fluidity being effected only by long and skilfully beating the mass with a whisk, or even with a spoon, seems to be produc'd but by pulling asunder the parts, (which perhaps before were long and somewhat twin'd) and breaking them into shorter or lesser, and consequently more voluble ones. And I remember, I have seen a good quantity of that jelly that is sometimes found on the ground, and by the Vulgar call'd a Star-shoot, as if it remain'd upon the extinction of a falling Star, which being brought to an eminent Physician of my acquaintance, he lightly digested it in a well-stopt glass for a long time, and by that alone resolv'd it into a permanent Liquor, which he extols as a specifick to be outwardly apply'd against Wens.

And here we will subjoin an Observation afforded us by the Art of Casting, which has sometimes yielded us a not unpleasant Diversion. 'Tis observ'd then by Gold-smiths well vers'd in that Art, (and has been recommended to me by an Artificer eminently skilful in it) as one of the chief Remarques belonging to it; that when any such curious work of Silver is to be cast, as requires that the impression of hairs or very slender Lines be taken off by the Metal, it is not enough that the Silver be barely melted, but it must be kept a considerable while in a strong fusion: For if it be too soon pour'd out, the figure it will make will be but blunt; whereas if it be kept a competent time in Fusion, the matter becoming thereby more Liquid as well as hotter, will be thin enough to run into the smallest cavities of the Mould, and so receive a figuration ev'n from the delicatest of them. Whence it may probably be deduc'd, that some Bodies already fluid may by a further comminution of their parts be made yet more fluid. The like increase of Fluidity may be observ'd in some other fluid Bodies, especially unctuous ones, as Turpentine, Oyle, &c. when heat begins to break as well as agitate their parts.

I may elsewhere have occasion to mention, how by the operation of the fire the Crystalline Salt of Urine may be reduc'd without Additaments to a strong and ponderous Liquor: though in this, as perhaps also in some of the former Instances, 'tis not unlikely that (as we may hereafter more particularly declare) there may concur to the produc'd change of consistence some alteration in the figure of the Corpuscles whereof the firm Body consisted.

And if that be true which *Helmont* in several places affirms of his prodigious Liquor, *Alkabeft*, it is possible to turn Plants, Animals, Stones, Minerals, Metals, or whatever kind you please of consistent Body here below, into a
Liquor

Liquor equiponderant to the resolv'd Concrete : which (if granted) seems to argue, That the most solid Body by being divided into parts small enough to be put into motion by the causes that keep those of water and other Liquors in agitation, may become fluid Bodies. And this Intimation I shall adde for the sake of Philosophers, that barely by long Digestions, (and much more if they be help'd by seasonably-repeated Distillations) in exactly stopt Vessels, and a due degree of heat, there may be made in the parts of many Bodies, both Vegetable and Animal, so great a change from the state of consistence to that of Fluidity, as those that contenting themselves with ordinary courses of Chymistry, have not had a peculiar curiosity for tryals of this nature, will not be forward to expect.

S E C T. XIII.

The Second of the above-mention'd three Conditions is, That there be store of vacant spaces intercepted betwixt the component particles of the fluid Body, or at least about those of them that are superficial : for without this there will not be room for each of the Corpuscles to continue its agitation upon the surfaces of the neighbouring ones ; and there would be no Cession of any, because there would be no place unpossess'd for the impell'd Corpuscle to be received in. But when I speak of vacant spaces, ordinarily, (if not always) requisite to be intercepted betwixt the particles of fluid Bodies ; I intend not to determine whether or no such spaces should or may be vacuities properly so call'd ; it being commonly sufficient to this second Condition of a fluid Body, that in the little spaces intercepted between those that either are, or at least are consider'd as solid parts, there be none but such as will easily yield to them, and cannot considerably resist the freedom of their motions.

Which being premis'd to keep this Condition from being mistaken, we may in confirmation of it take notice

how Snow, which at its first falling is of a loose and open texture, does easily yield to the impressions of the hand : But when by being strongly compress'd and form'd into Balls, the little Icy bodies it consists of are brought into a closer order, and many of them thrust into the little spaces formerly possess'd only by the yielding Air, they become unable to give way to the motions of our hand as before, but compose a hard and resisting Body. We see also, that when water is strongly forced into and kept compressed in a Bladder, so that its exterior particles have not about them as before the yielding Air to give way to them, when they should according to their wont swell about the sides of the Bodies that endeavour to press it inwards, it emulates a hard body, and resists such motions as otherwise it would readily yield to; unless a more easie Cession be occasion'd by the Retching of the moisten'd Bladder it self.

And I chuse to instance in a Bladder distended with water, rather than in one full of Air, because, though this latter will also emulate a hard Body, yet in this case the tension of the Bladder would perhaps be ascrib'd to a kind of Spring, which diverse Experiments have taught us to belong to the Air : whence it might be said, that since the enclos'd Air will suffer it self to be thrust inward a good way, though it will quickly when permitted flye out again; the hardness of a well-blown Bladder proceeds not from want of the rooms requisite to the Cession of the aerial Corpuscles, but to the motion of Restitution natural to them, when like an innumerable company of little Bows or Springs, being bent by the force that compresses the sides of the Bladder, they do as soon as it is taken off stretch themselves out again (some one way, some another) as far as is permitted them by the imprisoning bladder, which they thus every way keep strongly distended.

But

But this having of vacant spaces or some yielding matter about the Corpuscles of a fluid Body, seems requisite to its being so, but as what in a School-term one may call a *Removens prohibens*, I mean, only as it obviates that impediment to their motion, which exquisite fulness may be conceiv'd to give to the various glidings amongst themselves of the parts of a Body suppos'd to be perfectly of the same hardness or softness, or, if you please, altogether equally dispos'd or indispos'd to yield to one another. And although in such Bodies, as Water, Wine, Oyle, Quick-silver, and the like, that are generally agreed upon to be fluid Liquors, it will I presume be granted, that this second Condition we have been speaking of may take place; yet I will not say that 'twere altogether absurd to question, whether there may not be a portion of matter consisting of parts so minute, and so agitated, and consequently so easie to be either crumbl'd into yet smaller parts, or squeez'd into any figure as occasion requires, that they may incessantly change places among themselves, and thereby constitute a most fluid Body, without any vacuities, receptacles, or yielding matter about them, unless perhaps it be about the exterior parts of those of them that from time to time happen to be the superficial Corpuscles of this thinnest Liquor. But though we have said, that this may be question'd without absurdity, yet it will not so much concern us in this place to examine whether the affirmative may be rationally maintain'd, as to proceed to consider what is farther requisite to that state of matter we are now treating of, especially the Qualification yet unmention'd seeming to be the principal of all.

S E C T. XIV.

For the Third and Chief Condition of a fluid Body is, that the particles it consists of be Agitated Variously and Apart, whether by their own innate and inherent motion, or by some thinner substance that tumbles them about in

its passage through them. For this seems to be the main difference betwixt solid Ice and fluid Water, that in the one the parts (whether by any newly acquir'd texture, or for want of sufficient heat to keep them in motion) being at rest against one another, resist those endeavours of our fingers to displace them, to which in the other the parts being already in motion easily give way. For whereas in the Ice, every part actually at rest must by the Law of Nature continue so, till it be put out of it by an external force capable to surmount its resistance to a change of its present state; in Water each Corpuscle being actually (though but slowly) mov'd, we need not begin or produce a new motion in it, but only byass or direct that which it has already, which many familiar Instances manifest to be a much easier task. From this Agitation of the small parts of Liquors it comes to pass, that these little Bodies, to continue their motion, do almost incessantly change places, and glide sometimes over, sometimes under, and sometimes by the sides of one another. Hence also may be render'd a reason of the softness of fluid Bodies, that is, their yielding to the touch; for the particles that compose them being small, incoherent, and variously mov'd, it can be no difficult matter (as we lately intimated) to thrust them out of those places, which being already in motion they were dispos'd to quit, especially there being vacant rooms at hand, ready to admit them as soon as they are displac'd. And hence it likewise happens, that these little Bodies must be very easily moveable any way upon the motion of the mass or Liquor which they compose; and that being very small, and moving so many ways, they cannot but (according to *Aristotle's* Definition of things fluid) be very unfit to bound themselves, but very easie to be bounded by any other firm Body; for that hinders them from spreading any further: and yet to continue their various and diffusive motion as much as they can,

(especially

(especially their gravity, at least here about the Earth, equally depressing and thereby levelling as to sense their uppermost superficies) they must necessarily move to and fro, till their progress be stopt by the internal surface of the Vessel, which by terminating their Progress (or Motion toward the same part) does consequently necessitate the Liquor those little Bodies compose, to accommodate it self exactly (for ought the Eye is able to discern to the contrary) to its own figure.

SECT. XV.

This short and general Account of Fluidity may we hope be as well further explicated and illustrated, as confirmed, by the following Instances and Experiments, and therefore we shall forthwith proceed to Them.

And it will be fit to mention in the first place those that are afforded us by the Body our Author treats of, Salt-Petre, they having occasioned our writing about this Subject.

Salt-Petre then may be made fluid two several wayes, either by, or without a Liquor.

By the intervention of a Liquor it puts on the form of a fluid Body, when being dissolv'd in water or aqueous juices, it is not by the Eye distinguishable from the solvent Body, and appears as fluid as it; which seems to proceed from hence, that the agitated particles of the water piercing into the joints or commissures of the Corpuscles of the Salt, do disjoyn them, and thereby divide the Nitre into parts so small, that it is easie for those of the water, where-with they are associated, not only to support them, but move them to and fro: whence it comes to pass, that these Particles being so small, and swimming some one way, some another in the yielding body of water, make no such resistance against the motion either of a mans hand or other external Body that strives to displace them, as they did in their saline form.

But

But that with much less Liquor a Nitrous body may be rendred fluid, may appear to him that shall expose such fix'd Nitre as our Author teaches to make, to the moist Air of a Cellar : For there it will run *per deliquium*, (as Chymists speak) into a Liquor, which consists of no more aqueous Particles than are necessary to keep the saline ones (which seem to be much smaller than those of unanalyz'd Nitre) in the agitation requisite to Fluidity.

S E C T. XVI.

And hence we may proceed to consider, what Fluidity Salt-Petre is capable of without the intercurrence of a Liquor : and this may be two-fold. For first, if it be beaten into an impalpable powder, this powder, when it is pour'd out, will emulate a Liquor, by reason that the smallness and incoherence of the parts do both make them easie to be put into motion, and make the pores they intercept so small, that they seem not at a distance to interrupt the unity or continuity of the Mass or Body. But this is but an imperfect Fluidity, both because the little grains or Corpuscles of Salt, though easily enough moveable, are not alwaies in actual motion ; and because they continue yet so big, that both they and the spaces intercepted betwixt them are, near at hand, perceivable by sense. But if with a strong fire you melt this powder'd Nitre, then each of the saline Corpuscles being sub-divided into I know not how many others, and these insensible parts being variously agitated by the same heat, (both which may appear by their oftentimes piercing the Crucible after fusion, wherein they lay very quietly before it) the whole body will appear a perfect Liquor, and be thought such by any Beholder that shall judge of it but by the Eye : and such also is the Fluidity of melted metals, in which, when they are brought to fusion in vast quantities, I have seen the surface wav'd like that of boyling water, and sometimes parcels of Liquor thrown up a pretty way into the Air. And
not

not only Fire and other actually and manifestly hot Bodies are able to make some hard ones fluid, but it seems also that some bodies may be brought to Fluidity by others which to the touch appear cold, if they be but fitted to change the texture of the hard body, and put its inflected parts into a convenient motion; as may be seen in the Chymical Experiment of turning the brittle body of Camphire into an Oyl for the time, by letting it lye upon *Aqua fortis*, which perhaps bends and complicates the formerly rigid particles, and puts them into such a motion, that they do as well glide along as somewhat twine about each other. And I further try'd, (not having found it mention'd by the Chymists) that Camphire may by a dexterous application of heat be brought in close glasses both to flow and to boyl almost like Oyl. 'Tis true, that these Liquors taken from the fire quickly lose that name, and grow solid again. But the duration of a thing is not always necessary to denominate it such; for the Leaf of a Tree, for instance, whilst it flourishes, may be as truly green as an Emerald, though the leaf will after a while wither and turn yellow, which the stone will never do; and in cold Climates, where Lakes, &c. at other times navigable, are sometimes frozen so hard, that Carts and ev'n great Ordnance may safely be drawn over them, Ice and water are the one a stable, and the other a liquid Body, notwithstanding that the same portion of matter which at one time is frozen into a hard and solid substance, was a little before a fluid Body, and (now and then in a very short time) will be thaw'd into a Liquor again.

S E C T. XVII.

I know not whether it be requisite to take notice, that the Fluidity which Salt-Petre acquires upon fusion by fire seems very much of kin to that which is acquir'd by solution in water. But if fusion be made rather by the Ingress and transcursions of the atoms of fire themselves, than by
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the bare propagation of that motion with which the agitated particles that compose fire beat upon the out-side of the vessels that contain the matter to be melted; in such case, I say, both those kinds or manners of Fluidity newly ascrib'd to Salt-Petre will appear to be caus'd by the pervasion of a foreign body: Only in dissolution the fluid body is a Visible and Palpable Liquor, and consequently more gross, whereas in fusion the fluid substance that permeates it is more thin and subtil, and divides it into much smaller parts, and so adds very little to its bulk.

S E C T. XVIII.

But because some scruple may possibly arise about this matter from hence, that the powder of Nitre, how fine soever, seems fluid but just whilst it is pouring out, and ev'n then is but very imperfectly so; and that as for fusion, that is wont to reduce the melted body to a new and permanent state, as the formerly-mention'd powder of Salt-Petre, which before fusion was but a heap of incoherent particles, is by it made a solid and considerably hard Body: to prevent, I say, or remove such scruples, we will set down one Experiment that we long since met with, as to the main, in the shops of Stone-Cutters, which though unregarded by them, will excellently serve to make out what we mention it for. Take then good Alabaster, or in defect of this, of that white stone which is well known to our Masons by the name of Plaster of *Paris*, beat it very small, and put as many pounds as you please of the finely-searc'd powder into any flat-bottom'd (and first well heated) vessel of Brass or Iron (bigger or lesser according to the quantity you intend to burn:) Encrease the fire by degrees till it grow to be strong, and when the calorifick Atoms shall have in sufficient numbers pervaded the heap of powder, or, if you please, when the igneous Corpuscles have by their numerous and brisk strokes upon the vessel communicated by its means their agitation to the enclosed powder,
and

and when by either of these ways, or both, the fire (which may also resolve some of the more spirituous and exhalable parts (whereof Distillation has shewn me that Alabaster is not destitute) into Vapours) shall have put the little bodies it consists of into actual motion (which will be quickly done) you shall see it assume the form of a Liquor, and boil with numerous great and confus'd waves just like a seething-pot: and if, whilst it continues in this state, you stir it with a stick, it will not like a heap of sand, or as it self would do at another time, resist the motion thereof, but yield thereto like a Liquor, and, like it, will seem to have something of the Nature of a coherent body; for by stirring it any thing strongly near one side of the Vessel, you may make the waves beat very manifestly against the opposite part of it. And besides all this, you may observe this further resemblance betwixt this boiling matter and a Liquor, that there will flye up out of the Pot great store of steams like smoak, but that they are white, which will sometimes like smoak ascend, for ought can be discern'd, to the very top of the Chimney, and leave its colour upon the places by which plenty of it hath past. Besides, those that make this Experiment often, as we have taken pleasure to do, may have the opportunity to observe, that when the Vessel has continued so long over the fire that the contained Alabaster relapses into the form of a heavy moveless powder, by keeping it a while longer in the heat, it will for once at least resume the form of a fluid body, and boil again as before, the spirituous steams whose avolation promoted the Ebullition, being not yet quite spent. And lastly, if when it seems most a Liquor, you take up a little of it, and as nimbly as you can cast it upon a sheet of white Paper, it will not at all wet it, but immediately discover it self to be a moveless incoherent powder, as it was before its being set over the fire; whereby it (I hope) appears, that a heap or aggregate of such little bodies as are

neither Spherical nor otherwise regularly shap'd, nor small enough to be below the discernment of the Eye, may, without either fusion or being pour'd out, be made fluid barely by a sufficiently strong and various agitation (from what cause soever that proceed) of the particles that make it up, and lose its fluidity immediately upon the ceasing of it.

Thus have we seen how very much it conduces to the making of a Body fluid, that its small parts be actually mov'd. But whence this motion proceeds, we shall not at present venture to determine. For though in the Examples newly mention'd, and some others, most men will be forward to ascribe the motion produc'd in the parts of the fluid Bodies there mention'd, to the action of the fire whereunto they were expos'd; yet what it is that puts the parts of fluid Bodies in general into the motion requisite to make them such, is a Question of which the true Resolution indeed were very desirable. But the full debate of it will not, I hope, be here expected from me, whilst I am writing but Notes, since it would engage me to discuss two or three of the difficultest as well as the importantest Controversies belonging to Natural Philosophy. For first, I should be oblig'd to consider whether Motion, or a propensity to it, be an inherent Quality belonging to Atoms in general, and not losable by them; or whether all Motion is communicated by impulse from one Body to another. And since those that of late have taught that all visible Liquors, as Water, Oyl, Quick-silver, &c. owe their fluidity chiefly to the agitation of some thin and restless matter which incessantly permeates them, do deduce the necessity of such an Ethereal substance principally from the impossibility that there can be any *Vacuum* properly so call'd in the Universe, wherein yet are very many spaces unpossess'd by either Air or grosser Bodies than it: the Examination of this subtil matter would draw on the Consideration of the
nice

nice Controversies that perplex Philosophers concerning Emptiness, which 'twere more difficult for us to examine in few words, than it is necessary for us to meddle with them in this place; since not writing of the first Principles of Physiology, but of Fluidity, which is but a secondary or derivative quality (if I may so call it) it seems sufficient to give a notion of it, that we deduce it not from the unintelligible substantial form of the fluid Bodies, but from those simple and general Affections of Matter, the Figure, Situation, and Motion of its small parts.

S E C T. XIX.

Wherefore declining to adde any thing in this place to what we have elsewhere discoursed concerning the Origine of Motion, and the possibility or impossibility of a *Vacuum*; we will proceed to take notice that there is one thing more which we may learn from Salt-Petre touching the Nature of Fluidity, and that is the distinction betwixt a fluid Body and a wetting Liquor, which are wont, because they agree in many things, to be confounded, but inconsiderately enough: for though every wetting Liquor be fluid, yet every fluid Body does not wet. The Air, the *Æther*, and ev'n flame it self may be properly call'd Fluid Bodies according to the notion of Fluidity hitherto made out, and yet will scarce by any man be call'd Moist Liquors; and Salt-Petre, whilst in fusion, is really a Liquor, and so is every melted Metal, and yet these wet not the bodies they touch, as do Water and other wetting Liquors, which are fluid bodies with this peculiar qualification, that they stick to and moisten the dry bodies which they touch (or at least abound with some parts, which being separated from the rest and reduced to a Liquor, will do so.) And according to this notion, methinks, it may be conceiv'd, that the humidity of a body is but a relative thing, and depends chiefly upon the congruity or incongruence of the component Particles of the Liquor in

reference to the pores of those particular bodies that it touches : for, sometimes the little eminencies and pores of the surface of the dry body on or against which the Liquor flows, are of such magnitudes and figure, that the particles of the Liquor find admittance into those pores, and are detain'd there (by which means they usually soften it ;) and sometimes the pores and asperities of the dry bodies surface are so incommensurate in bigness & figure to the particles of the Liquor, that they glide over the surface without sticking or adhering firmly to any part of it. This may be exemplifi'd in Quick-silver, which cannot be said to be a humid body in respect of our hands or cloaths, or of almost all other bodies of the World, upon whose surfaces it will roul without leaving any of its particles lodg'd in their pores, or fastn'd to their little eminencies, whence it is called by vulgar Chymists, the water that wets not the Hands : but in reference to divers Metals, especially Gold and Tin, Quick-silver may be said to be a humid Liquor, for it insinuates it self into their pores, and thereby mollifies their bodies, as other Liquors do those that are moistened by them. And even water, that wets almost all other Animal and Vegetable, and many mineral bodies, besides that it is commonly enough observ'd to stand in almost globular drops upon Cabbage-leaves, seems not a humid Liquor in relation to the feathers of Ducks, Swans, and other water-fowl, whom Nature having design'd to flye sometime in the Air, and live sometimes in the water, she providently makes their feathers of such a texture, that they do not, like the feathers of divers other birds, admit the water, which imbib'd would make them unfit for the use of flying. And 'tis observable, that upon the change of texture in a Liquor, it may be brought to stick to the surface of a body to which before it would not adhere ; as may appear by this, that though Quick-silver alone will not stick to glass, yet if there be mixt with it a due proportion

tion of Lead, Tin, and Tin-glass, though neither of them will adhere to glass, yet their liquid mixture (as we have often tryed and elsewhere * taught) readily will, even without the assistance of heat.

* The Writing here referred to, is the Second Tome of the Usefulness of Experiment. Philosophy, which should have appeared before these Specimens.

S E C T. X X.

If it be objected, that this various agitation of the insensible parts of water and resembling bodies wherein we make the Nature of Fluidity chiefly to consist, is but an imaginary thing, and but precariously asserted, since by our own Confession they are so small, that the particles themselves, and more, the diversity of their motions are imperceptible by sense, which represents water, for Example, to us as one continu'd body, whose parts are at perfect rest ;

If this, I say, be urged against our Doctrine, we shall not deny the Objection to be plausible, but must not acknowledge it to be unanswerable.

For of the seeming continuity of Water and other Liquors this may be the Reason, That the particles whereof the Liquor consists, being too small to be visible, and being not only voluble, but in actual motion, the pores or vacant spaces intercepted between them, must also be too little to be discern'd by the Eye, and consequently the body must appear an uninterrupted or continu'd one : not to mention, that, were the parts of the Liquor less minute, their shifting of places would hardly be perceiv'd by the Eye, each displac'd Corpuscle being immediately succeeded by another like it. 'Tis true, that a heap of grains of Nitre, though upon its effusion out of the Vessel it somewhat emulates a fluid body, does yet when it rests in the Vessel appear to be but an aggregate of many little incoherent bodies heap'd up together ; because the intervals or holes left between them are great enough to affect the sense :

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But if the same Salt be reduc'd into an Alchoole (as the Chymists speak) or impalpable powder, the particles and intercepted spaces being then extreamly lessen'd, the body they make up will much more resemble an intire mass, though it be look'd upon from a nearer distance; and so when this powder is by the fire further broken into parts incomparably smaller than those of the powder, and which consequently intercept such extreamly little pores, that not only Salt-Petre, but some Metals, and ev'n Gold it self, (from which it will not be suppos'd that any thing exhales to lessen it) are *by some* affirm'd (for I have not my self diligently enough observ'd it, and do yet doubt it) to take up rather less than more room melted than cold, why should we not grant that these pores may be little enough, not any where to discontinue the body as to sense?

S E C T. XXI.

And that the incoherent parts of fluid bodies are also diversly agitated, some this way, and some that way, though the sense cannot discern it, may be prov'd by their sensible operations. * For without such

[* The Author now finding that something concerning the various motion of the parts of fluid Bodies, which he has but touch'd upon, has been, though but briefly and without Experiments, yet excellently explain'd in a mathematical way by Monsieur des Cartes in the 56th and 57th Articles of the second Part of his Principles, thinks fit to refer the inquisitive Reader thither for fuller satisfaction about that Particular.]

local motion, how could the particles of water pierce into the recesses of Bodies, and occasion those putrefactive alterations that are wont to be imputed to superfluous moisture? And how comes it else to pass, that aqueous Liquors so readily diffuse themselves into, and so exquisitely mingle with one another? as we see when red and white Wine are in a trice confounded into Claret: and without this various agitation of the parts of water, how could it be that lumps of Sugar or Salt cast into it, should quickly be so perfectly dissolv'd in it, that the lumps themselves totally disappear, and the dis-

sociated

fociated parts are carried about every way by those of the water, even from the bottom to the very top? as is evident particularly in Sea-salt, which when the superfluous Liquor is sufficiently exhal'd, begins visibly to coagulate, not at the bottom, but upon the surface of the water; and not only Salt, but even Gold it self, though the heaviest of bodies, may have its parts so scatter'd by the agitation of those waters, as Experience has taught us, and as you may easily try by putting a little of the Solution of Gold made in *Aqua Regis* into 15 or 20 times as much fair water, which will all thereby be immediately enobled with a Golden Colour. That the little bodies whereof flame consists are fiercely agitated, appears oftentimes even to the Eye, and will scarce be denied by him that considers the operations of it, and the vivid beams it darts round about it against the neighbouring bodies. And that the particles that compose our common air are also very diversly agitated, we may be induc'd to believe by sundry particulars. As first, by those little moats that from a shady place we see swimming up and down in the Sun-beams, and by the tremulous motion which that of swarms of little bodies in the air seems to impart to distant objects look'd on after Sunrise through a good Telescope, (and which by the bare Eye in hot weather may be often discover'd by certain very dilute shades, which seem to tremble upon the walls of high-roof'd Halls and Churches, and other spacious Buildings.) Next, (and more easily) by this, That if you take Salt of Tartar, first brought to fusion, and place it in a Cellar, or ev'n in an ordinary Room, it will in a short time (now and then in a few minutes) begin to relent and have its surface softn'd by the imbib'd moisture of the air, wherein if it be left long enough, it will totally be dissolved into clear Liquor; which would not be, if the moist vapours that help to constitute the air did not move to and fro every way, and were not thereby brought to the
Salt,

Salt, and enabled to insinuate themselves into its pores, and by that means dissolve it, and reduce it with themselves into a Liquor.

And even in Summer, when the air is wont to be much dryer than at other Seasons of the year, one may quickly discover that there are in the air store of aqueous Corpuscles, mov'd some one way and some another, by the Experiment of putting into a Drinking-glass, for want of Ice and Snow, some Beer or Wine actually very cold: for thereby, after a while, the outside will appear all bedew'd with little drops of Liquor; which seems plainly to be no other than the aqueous steams that swimming up and down in great multitudes in the air, are by its agitation towards all parts carried, as every other way, so to the sides of the Glass, and being there condens'd by the coldness of that smooth Body, turn into visible and palpable water. And, if I much mis-remember not, it was one of the circumstances of the last Experiment of this kind we have had occasion to take notice of, That the drops that fastn'd themselves to the outside of the Glass, purposely left in part unfill'd, reach'd either not at all, or very little further than the surface of the Liquor within the Glass, whose coldness as it seems did not infrigidate those upper parts of the Glass, to whose level the Liquor it self did not reach. To which I could easily adde Arguments to prove, that the drops we have been speaking of proceeded not from the transudation of the Liquor within the Glass, if I thought it worth while to disprove so unlikely a Conjecture. But instead of that I shall only intimate, that from this Experiment useful hints may be taken both Theoretical and Practical, and particularly that a Reason may perchance be given of a strange way of catching a Salt and Liquor out of air, barely by glass-vessels of a peculiar and skilful contrivance. Much of what we have lately said will, I presume, be the less wonder'd at, if we subjoyn what

Expe-

Experience has taught us, That 'tis not difficult by the help of a convenient Furnace and fit Vessels to make that ponderous Metal, Lead, ascend to a good height in the open air, in the form of a copious smoak: such a smoak we discern'd after a while to be carried so many ways by the aerial Corpuscles that it met with in actual motion, that it was soon dispers'd so far as to disappear: which perhaps will be thought some confirmation of what we formerly deliver'd, when we taught how much the being divided into very minute parts may conduce to the Fluidity even of ponderous Bodies.

S E C T. XXII.

And though Quick-silver be, excepting Gold, the heaviest known body in the world, yet when it is reduc'd into vapour, it seems to be carried to and fro like the other terrestrial particles that swim up and down in our air: for I remember, that an expert Gilder not long since complain'd to me, that if when he evaporated Quick-silver, he forgot to take off his Rings from his hand, though they touch'd not the Quick-silver whilst it was in a body, the roving fumes would oftentimes fasten upon the Gold in such plenty as would put him to much trouble to get them off from his Rings; one of which he shew'd me that he had lately thus whitened, and as it were silver'd over with Mercurial fumes, and was then to restore to its native Yellow.

S E C T. XXIII.

But let us return to visible Liquors, and endeavour to prove almost *ad Oculum*, as they speak, that their insensible parts may be every way agitated, though their motion be but seldom visible to us. Take then what quantity you please of *Aqua fortis*, and dissolve in it as much as you please of ordinary coyn'd Silver, (it not being necessary for this Experiment that it be refin'd) and pour the coloured solution into 12 or 15 times as much fair water, and

then decant or filtrate the mixture, that it may be very clear. If you look upon this Liquor, the parts of it will seem to be all of them as perfectly at rest, as those of common water; nor will your Eye be able to distinguish any Corpuscles of Silver swimming in the Liquor: and yet that there are such metalline Corpuscles agitated to and fro with and by those of the water will quickly appear, if you immerse into it a flattened piece of clean Copper, for by that time you have held it two or three minutes of an hour (perhaps not so long) in the Liquor, you shall see the particles of Silver that were roving up and down the Liquor, fasten themselves in such swarms to the Copper-plate, that they will appear in their native hue, and cover it, as it were, with a loose case of Silver, which may be easily shaken off in the form of a metalline powder: and if several such Plates be left all night, or for a competent number of hours, in the bottom of the Vessel, you may the next day find all the particles of Silver that were dispers'd through the whole body of the Liquor, settled upon or about them; the deep blewish green tincture you will discover in the water proceeding only from some little parts of the Copper-plates, and of the Alloy of the Coyn, dissolved by the saline particles of the *Aqua fortis*. And I remember, that to compleat the Experiment, I have sometimes made even these fall to the bottom of the Vessel, by leaving a lump or two of Spelter there for two or three days: for, not only those metalline Corpuscles that were just over or near to the determinate place where I put the Spelter, but also all the rest, into how remote parts soever of the Liquor they were diffus'd, did settle upon the Spelter, as appear'd both by its increase of bulk, and by their leaving the water clear and colourless; which plainly seems to have proceeded from hence, that the particles of the water were restlessly and every way agitated, and so by frequently gliding along the surface of the Spelter they
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must carry thither of the Corpuscles of Copper mingled with them, some at one time and some at another, till at length all were brought to it and detain'd there.

S E C T. XXIV.

That of the particles of Spirit of Wine, and such like inflammable Liquors drawn from fermented Juyces, though they seem to the Eye to be at rest, a good many do yet move confusedly and very nimbly, I remember I have long since manifested by an easie and ocular proof which I devis'd about 10 or 12 years ago, when (being yet scarce more than a Boy) I first began to consider what Fluidity might be. The Experiment as I writ it down, with all the Circumstances and Observations relating to it, I have not now by me; but having divers times been desir'd to shew it to Learned men (Physitians, Mathematicians, and others) I cannot have forgotten those Phænomena of it that are the most pertinent to our present Subject. Supposing then that in pure Spirit of Wine, beside the aqueous parts that glide softly along each other, there are store of volatile and Spirituous Corpuscles, whose agitation is stronger, I let fall (from a pretty height, that it might be broken into small drops by its fall) into any wide-mouth'd glass fill'd with this Liquor, (which must not be over-dephlegm'd, lest the Oyl sink in it) a little common Oyl or Spirit of Turpentine, which I therefore made choice of, because its tenacity, greater than that of the Chymical Oyls of Spices, makes it that it will neither mingle with Spirit of Wine, nor spread it self, as divers other distill'd Oyls will, upon the surface of it, but keep it self in the form of round drops, whose shape facilitates their motion. The Oily drops then swimming at the top of the Spirit of Wine, will be, by the disorderly roving of the agile parts of it (which hit against them little Globes, as the vivous Spirits ascend to exhale) made to move restlessly to and fro in an irregular manner, the drops sometimes bearing up to

one another, as if all or most of them were presently to unite into one body, and then suddenly falling off, and continuing to shift places with one another, after a manner pleasant and strange enough to them that never before saw the Experiment: and this dance will continue for half an hour or an hour, (or a shorter or much longer time, according to the quantity and strength of the Liquor) till the spirituous parts being flown away, the drops being no longer impell'd lye at rest upon the disspirited Liquor, as they would upon common water. And whereas the nimble motion of the drops might be suspected to proceed from some secret contrariety in Nature betwixt the Oyl of Turpentine and Spirit of Wine; besides that I could easily shew that those two Liquors have no Antipathy, I not only try'd the Experiment with another inflammable Liquor than Spirit of Wine, but (if I much misremember not) found, as I expected, that little pieces of chop'd straw (such both being light and not easily imbibing moisture) being gently let fall upon the Spirit of Wine, were in a tumultuous manner carried to & fro upon the surface of it; though I am not sure but that the motion of the Oily drops may be in part due to some partial solution made of them by the vivous Spirit, which during that action may tumble them to and fro; not to add that I have by some tryals, been tempted to suspect the air may have some interest in the motion of the drops. However, I have mention'd the recited Experiment, not as if I thought that either it or fugitive Spirit of Wine were fit to teach us the nature of fluid Bodies in general, but to shew by an ocular example that there may be a quick and intestine motion in some parts of a Liquor, notwithstanding that the unassisted Eye can discern no such matter. I shall not here relate, how having caus'd to be Hermetically seal'd up some of these Liquors in a glass, to try how long the extravagant dance of the drops would last, when the

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more spirituous parts of the vinous Liquor could not exhale, my vessel was soon broken without any discernable violence. Nor shall I now take notice of any of the other Phænomena of our Experiment, partly, because I have elsewhere mention'd most of them; and partly, because I think it more pertinent to our present Theme, to observe that this unseen agitation of the minute parts will not only hold in light and spirituous Liquors: For, that the insensible parts even of the heaviest Liquors themselves are also in actual motion, though many may think it unfit to be believed, will follow from what has been already deliver'd concerning the nature of fluid bodies, as such; and may be confirm'd by this, that whereas three of the heaviest Liquors we yet know of, are Quick-silver, Oyl of Tartar *per Deliquium*, and Oyl of Vitriol, that first-nam'd will even in the cold penetrate into the pores of foliated Gold, and destroy the texture of that closest of Metals; the Liquor also of Salt of Tartar will in the cold draw tinctures from several bodies, and we have endeavour'd to evince the agitation of the parts of Oyl of Vitriol, not only by shewing how in the cold it would corrode divers Metals, but by casting little pieces of Camphire into it, which without the assistance of the fire were made liquid by it, and appeared like so many drops of Oyl. And he that yet doubts, whether the parts of these two Oyls (as Chymists abusively call them) how ponderous soever they be, are fiercely agitated or no, may probably be soon satisfied by shaking an ounce or two of each of them together, and observing the heat, hissing, ebullition, and sparkling, that will suddenly ensue upon their being blended.

S E C T. X X V.

But here we must take notice, that though it belong to the Nature of fluid Bodies, that their parts do easily shift places; yet that is to be understood only as to the parts of the same fluid Bodies, as water, or of such differing fluid
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Bodies as are dispos'd readily to admit each others particles, and mingle together, as we see in Water and Wine. For otherwise, fluid Bodies may be of such differing natures, that when two or more of them are put together, they will not mix, but each retain its own distinct surface; so that in regard of one another, the contiguous Bodies do in some degree emulate each of them the Nature of a consistent Body: for though it cannot be look'd upon as a hard body but a soft, because of the easie Cession of its superficies, yet it does like a compact or consistent body deny to mingle permanently with the contiguous Liquor or other fluid substance. And I somewhat wonder, that *Lucretius* and other Atomists should (at least for ought I remember) over-see this Observation, since it is obvious enough in Oyl, which will not mix with water, but float upon its surface: Not to mention, that Quick-silver will not incorporate with any of the familiar Liquors known to the Ancients. I had almost forgot, that I promis'd at the beginning of this Discourse an Instance concerning Flame, which I will therefore now recite. And it is, That having by an easie preparation of Copper, by the intervention of a little Sal Armoniack, (which I have already taught in another Treatise*) so open'd the Body of that Metal, as to make it inflammable; I took some small grains of this prepar'd Mineral, and put them under the wieck of a strong and actually burning Candle, whereby (as I expected) they were with the melted tallow soon carried up to the bottom of the flame, and by it so kindl'd, that the green (not blew) flame of the cupreous Body did (somewhat to the wonder of the Spectators) burn for a good while (this combustibile matter being marvellously lasting) distinct from the yellow flame of the Candle, as if there had been some invisible partition between them. But to return to the unminglable Liquors we were formerly speaking of; the

* *An Historical Dialogue about Flame.*

the cause why these retain their distinct surfaces, my present task does not oblige me to enquire into: but this I shall observe in general, that it seems to depend very much upon the texture of the particular Liquors, and perhaps too upon the peculiar motions of their minute parts. For I have observ'd, that though pure Spirit of Wine and Salt of Tartar, resolv'd into a Liquor by the moisture of the Air, will, when put together, retain distinct surfaces, or presently regain them if you shake the Liquors never so strongly together; yet by adding a little fair water to either of them, the texture being thereby alter'd, it will easily incorporate with the other. And thus although that (as I noted already) common Spirit or Oyl of Turpentine will not mingle with Spirit of Wine, yet having had the curiosity to make a tryal with Oyl of Turpentine abstracted skilfully, and with a very gentle fire, (for otherwise the Experiment may easily miscarry) from melted or at least well decrepitated Sea-Salt, we found, according to expectation, that though there appeared no visible alteration in the Oyl, yet we could easily by shaking confound it with pure Spirit of Wine. Moreover, though lixivate Liquors and Oyls will not by an ordinary Agitation be permanently joyn'd, yet I have try'd, that by digesting a good while a solution of Salt of Tartar with Oyl of Almonds, I could reduce them to a soft Saponary substance: which Experiment makes somewhat more to my present purpose, than the common practice of Sope-Boylers, because I did not, as they, boyl away the water wherein the lixivate Salt is dissolv'd. I might adde also, that if you put one part of Quick-silver into about four parts of Oyl of Vitriol, you will find (at least if the Experiment proceed always after the same manner as it has done with me) that the two Liquors will remain distinct whilst you keep them in the cold, but if by degrees of heat you bring the Oyl of Vitriol to boyl, it will pierce the surface of the Quick-silver, and
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by partly incorporating with it, reduce it to a substance very differing from what it was. But because these two last Experiments may be with less improbability than the two that preceded them refer'd to other causes, I shall no farther insist on them, but take notice of one thing more concerning the difference of fluid Bodies. And it is this.

S E C T. XXVI.

I observe, that some of them do not only not mingle with others that are contiguous to them, but fashion the surfaces of those others, and reduce them to determinate shapes. This I have taken pleasure to consider in some Chymical Liquors, which I have purposely put together; for Instance, having pour'd Spirit of Wine upon Oyl of Tartar *per Deliquium*, (as Chymists call it) I found that the superficies wherein they touch'd each other was flat, or (as to sense) parallel to the Horizon. But if this were done in a very slender or narrow glass, with the mouth unstop'd, though the lower surface of the Spirit of Wine which touch'd the other Liquor appear'd very level; yet the upper superficies, which was contiguous to the Air, was manifestly very concave. And if to these two Liquors I did in a broader glass pour Oyl of Almonds, that Oyl would sink to the bottom of the Spirit of Wine, (that being well rectifi'd) and floating upon the Oyl of Tartar, would separate the two Liquors, and both above and below retain a flat or level surface. But if instead of Oyl of Almonds, or another express'd Oyl, I dropt into pure Spirit of Wine, swimming upon Oyl of Tartar, some common Oyl of Turpentine; the Oyl would gather into parcels, (some of the bigness of hail-shot, some as big as small Pistol-bullets, and some of other sizes) which in case they did swim in the Spirit of Wine, and touch'd neither of its surfaces, seem'd globulous, and continu'd so (the glasses being stop'd) for many hours: But in case they emerg'd to the upper part of the Spirit of Wine, as much of them as lay
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immers'd in the Spirit (which was by far the greatest part of them) appear'd round, and continu'd so long as I pleas'd; the upper parts only of those little globes appearing to have the same surface with the Spirit of Wine. And I further observ'd, that some small drops would as it were rest constantly upon the superficies of the Oyl of Tartar, touching it but as it were in a point, and continuing to the Eye Spherical; though the surface of the Liquor were purposely now and then somewhat shaken. But that which I took special notice of was, that having (upon design) into pure Spirit of Wine (for upon common Spirit Oyl will swim) let fall some great drops of Oyl of Turpentine, they did at first sink to the bottom of it, and lie upon the surface of the Oyl of Tartar almost like Hemispheres, whose convex part was all above the Oyl of Tartar; but after a while they were, as I expected, press'd on all sides and fashion'd into round Bodies (yet a little more protuberant at the sides than the top) which seem'd scarce to touch the surface of the Oyl of Tartar on which they lean'd. Diverse other Observations of this kind were afforded me by some peculiar mixtures that I made of Chymical Liquors: But not having the leisure to set them down, much less to enquire into their causes, I should scarce have mention'd what I have already deliver'd (especially since we found that a light variation of Circumstances would often alter the event of such tryals, which we have therefore set down barely historically) but that finding that drops of Water, Quick-silver, and other fluid Bodies, seem'd to be fashion'd into a round figure by that every way almost equal pressure of the ambient Air; and having likewise try'd, that Quick-silver suspended in the Air (as it may easily be, if the Torricellian Experiment being made in a tube exceeding slender, some Air be afterwards dexterously let in to divide the long Mercurial Cylinder into diverse short ones) has both at the top and

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bottom,

bottom, where it is expos'd to the Action of the Air, a very protuberant surface; finding, I say, these Effects of fluid Bodies upon one another, I thought it not amiss to intimate, how some Experiments might be made that may possibly facilitate the giving an Account of the figuration of some of the more considerable fluid Bodies, which, as we noted already, make up much the greater part of the Universe: especially, since I could easily enough make it probable, that such Steams of the terrestrial Globe as may well be suppos'd to be the chief Ingredients of our Atmosphere, may like a Liquor retain a superficies distinct from that of the ambient and contiguous Body. And since we are speaking of the distinct surface of fluids, the occasion invites me to add an Experiment, which, though apt to miscarry upon the account of unheeded Circumstances, has yet, when all things were rightly order'd, succeeded very well: I will, I say, subjoin it here, because it shews a way of dividing in a trice, a Liquor Transparent, and, as to sense, Homogeneous into two very differing Liquors, the one Diaphanous, and the other Opacous, which will not mingle. The Experiment is this; Dissolve one Ounce of clean common Quick-silver in about two Ounces of pure *Aqua fortis*, so that the Solution be clear and total, then whilst it is yet warm, pour into it by degrees, lest they boyl over, half an Ounce or one Ounce of Filings of Lead, and if no Error, nor ill Accident have interven'd, the Lead will be in a trice præcipitated into a white Powder, and the Mercury reduc'd into a Mass (if I may so speak) of running Quick-silver, over which the remaining part of the *Aqua fortis* will swim, whereby we may see that Liquors being reduc'd into very minute parts, may mingle very well, the Corpuscles of the one supporting in that state those of the other, though in greater Bulk, especially, the Texture of one being somewhat varied, they will retain distinct Surfaces. *N.B.* Note, that when the Operation

ration succeeds not well, the Mercury need not, for all that, be lost, but may (in great part at least) be recover'd by freeing the Præcipitated matter from the rest by filtration, and then diligently grinding the white Præcipitate with Water, by which means, the Mercury will little by little be gathered into drops. And though this be far from being the true Mercury of Lead, as I may elsewhere shew you; yet some Inducements, not here to be named, incline me to look upon it, as somewhat differing from common Mercury, and fitter than it for certain Chymical uses.

SECT. XXVII.

And here I should pass on to the Consideration of Firmness; but that when a while ago I discours'd of the Agitation of the Corpuscles that compose Oyl of Tartar and Oyl of Vitriol, I forgot to add, that not only in fluid bodies, but in some also of those that are consistent, there may perhaps be more motion in the insensible parts than our senses discern, or we are wont to imagine: especially in those bodies, which having been once endowed with life, are, though not fluid; yet either soft, or at least not perfectly hard. I have more than once taken pleasure to look upon an heap of swarming Bees, for though they make not up a liquid but coherent body, which may be turn'd upside down without losing its coherence, and which being beheld at a distance, seems to be one entire mass or body; yet it is evident to him that looks at them near enough, that the particular Bees that swarm have most of them their distinct and peculiar motions, and that yet these motions of the particular Bees destroy not the coherency of the heap; because that when one of the more innermost Bees removes, as she lets go her hold from those that she rested on before, and goes away from those that rested on her, so she meets with others on which she may set her feet, and comes under others that in like man-

ner set their feet on her, and so by this vicissitude of mutual supports their coherence and their removes are made compatible; and if instead of Bees, the swarm consisted of extreamly little flies, their particular motions would perhaps be inconspicuous. And that some such thing may happen in the consistent bodies we have been speaking of, seems probable from hence, that in wainscot and other hard wood, we often see little heaps of dust produc'd in them by putrefaction; and not only in Cheese we many times see multitudes of mites start up, but in Apples and other Fruits we oftentimes find Magots, though the skin be whole, (which could not be unless the parts of the matter were variously transpos'd, (that is, put into a local motion) and connected after a manner suitable to the Nature of the insect to be produc'd:) And by the growth of bones in the bodies of perfecter Animals, as well in respect of the internal cavity, where the marrow lodges, as of the external surface, as also by the growth of the shells of Oysters and Snails, (though cold Animals) from a size inconsiderable at first, in regard of what is afterwards attain'd to, and by some other resembling particulars, it seems that the small particles that constitute even the solid parts of Animals are not, whilst the Creature lives, (or at least whilst it grows) altogether exempt from some (though slow and insensible) local motion. And I remember, that it has by a very diligent observer been affirmed to me, that he saw several pieces of Gum swet out of an old wainscot of above twenty years standing. Which I the less wonder at, because I have several times seen viscidous Exsudations disclose themselves like drops of Turpentine upon Deal-boards, which had been made use of about Buildings. (But of this subject more (perhaps) elsewhere.)

SECT. XXVIII.

After we have hitherto discours'd of Fluidity as considerd

der'd in distinct Bodies, we might properly enough say here something of what furtherance or hinderance in respect of Fluidity one Body may receive by being mingl'd with another. But the consideration of those changes of Consistence which may be produc'd by Mixture, is a Subject that we shall have such frequent occasions to treat of in what we are to deliver about Firmness, that we shall now only give this general Admonition, That 'tis not so safe as one would think, to fore-tell the consistence of a mixture of two or more Bodies, from the bare consideration of the consistence of those Bodies whereof it is to be compounded. And that we might at once both manifest this, and insinuate what Judgment should be made of what is said by so many Chymists and others, who without Limitation teach, That the Addition of Salts to metalline and mineral Bodies does much facilitate their fusion, I remember I purposely made and employ'd this Experiment.

We dissolv'd crude Copper in a due quantity of Spirit of Nitre, and by Evaporation reduc'd the Solution to a kind of Vitriol of a lovely colour. We also corroded with two parts of Spirit of Nitre one of good Tin, and suffer'd the mixture to reduce it self (as it easily did) to a substance almost like Meal. Of this mixture we put a parcel into a Crucible, and suffer'd it to grow (and for a pretty while to continue) red hot: Nay, we put some of it upon a quick coal, and excited the heat by frequently blowing the fire, without finding that this metalline meal did at all melt, though Salt-Petre be a fusible Salt, and Tin it self be of exceeding easie fusion. Whereas, although Copper be a metal which is much harder to melt; not only than Tin and Lead, but even than Silver, (as those well know that mix Silver with Copper to make a fusible mixture to solder upon Copper and Brass) yet was this metal, that will endure a long and strong Ignition by
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being joyn'd *per Minima* with the same kind of Nitrous Corpuscles, that had so contrary an effect upon the Tin, so strangely dispos'd to fusion, that the Vitriol would melt with as small a heat as that of ones hand. Nay, we have made such a Vitriol either with Spirit of Nitre or with (what differ'd little from it) a certain *Aqua fortis* as would even in Winter be made to stand melted for divers hours together, by the languid warmth of the Sun, though shining on it but through a window, where it also stood but in an unstop'd Glass. So fit it is that we consider as well the new Texture that mingl'd Bodies obtain by the association of their particles, (whose size and shape, and perhaps motion may be thereby much alter'd) as the consistence of the particular Bodies before their being mixt.



THE HISTORY OF

Fluidity and Firmnesse.

The Second Part.

OF FIRMNESSE.

IT is sufficiently known that the Chymists ascribe the Firmness and Hardness of Bodies to Salt, and teach that the Saline Ingredient of them is the Principle of Coagulation in them, and the cause of their Compactness and Solidity. But though this opinion of the Chymists be embrac'd by so many modern Philosophers and Physitians, that some may think it superfluous to make enquiry after other Causes, yet others (to whom the Explications of Chymists seem not always so much as Intelligible) will upon the very account of the Receivedness of the propos'd Opinion, think it rather worthy to be examined than to be acquiesc'd in. However, without making it our business, either to side with, or oppose any Sect of Naturalists, we will apply our selves a while to consider the thing it self in prosecution of the Design already begun. And having in the fore-going
Part

Part of our little History taken a general view of Fluidity; we will now proceed to try what Light it will afford us to discover the nature of Firmness or Compactness.

And since fluidness and stability being contrary qualities, are to be apprehended under contrary notions, we may conceive that the firmness or stability of a body consists principally in this, that the particles that compose it, besides that they are most commonly somewhat Gross, either do so rest or are so intangled between themselves, that there is among them a mutual cohesion whereby they are rendred unapt to flow or diffuse themselves every way, and consequently to be, without violence, bounded and figur'd by other surfaces than those which their connexion makes themselves constitute.

In this rude Description of Firmness we have intimated Three principal causes of it, namely the Grossness, the quiet Contact, and the Implication of the component parts.

The first is Grossness of Parts, of which we have in effect almost sufficiently discours'd already: for since treating of Fluidity, we manifested at large how conducive smalness of Parts was to that Quality; 'tis easie to deduce that Grossness of parts in a Body must commonly be a great disposition to its being Firm. And bigger Corpuscles being *ceteris paribus* more difficult than lesser to be put into motion, when they are once at rest, it is obvious that a Body consisting of such Particles is less dispos'd to become fluid, and consequently more apt to continue firm, than if its component parts were smaller, and thereby more easie to be displac'd. But when I speak of the Grossness of Corpuscles, I pretend not to determine whether or no Body or Matter be so perpetually divisible, that there is no assignable portion of matter so minute that it may not at least Mentally (to borrow a School term) be
further.

further divided into still lesser and lesser parts: For allowing this indefinite Divisibility of corporeal substance, 'tis plain that it may in some sense be averr'd, that there are no firm Bodies whose Parts are not extreamly minute. But I understand by the gross parts I here speak of, such Corpuscles as actually convening to constitute a Body, are scarce dissipable or divisible into lesser by the Agitation of the ambient Air or Æther, or by the other causes of the Fluidity of Bodies.

It is also to be noted, that when I spake of the fitness of grosser Corpuscles to make a firm Body, I added, *Cæteris paribus*, because it may happen that the breaking of the small parts of a Body into minute Particles may make them but the fitter to contribute to the Firmness of the Body they belong to: For the parts of the divided Corpuscles may by their comminution acquire a new, and perhaps a more irregular shape than before, upon whose account they may be more dispos'd to be entangled among the neighbouring Particles, or may be better fitted to get into and fill the pores of some kind of Bodies. And in such little Particles, not only the minuteness may make them lie closer together, and consequently the better exclude the Air: but the greatness of the surface in proportion to the bulk of the matter may perhaps in some Cases occasion a fuller contact, and so facilitate the constitution of a very firm Body, in case these minute parts (whose intervals (if they intercept any) need not be other than very small) shall be placed and disposed to the best advantage for a full contact of one another. But as I said, a while before, from what we have already delivered (concerning the size of parts, when we treated of Fluidity) it may easily be understood how much the magnitude of them may conduce to Firmness; and therefore we will presently pass on to the mention of the two other things to be considered in reference to consistent Bodies. Where-

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of the one is the bare Rest of the small and contiguous parts that make up the firm Body ; and the other the intricate Texture of such parts in the body they make up. And either of these two seems alone sufficient to render a body stable : though Nature do perhaps oftentimes make some (though not equal) use of both, to fasten the parts of the same body more firmly together.

Of the former of these Causes, I am inform'd that the justly famous Monsieur *Des Cartes* has also taken notice, but without adding proof from Experiments, or Observations. But it seems to have been either over-look'd, or, (as incongruous to their Hypothesis of the innate motion of Atoms) rejected by the old Atomists, and by *Lucretius*, who takes notice (that I remember) only of the latter : for though they did of old make mention of the sudden divulsion of two flat and solid Bodies, yet they employ that Observation but to prove a Vacuum, (without otherwise taking notice, that I have met with, of those things that are most material in such Observations to our present purpose, and without deducing thence what we shall endeavour to do in order to the explication of the causes of Firmness.) Upon what account then soever the Atomists have omitted to reckon for a cause of Firmness that which we have newly been speaking of ; yet (as we observed above) If two bodies be once at rest against one another, it seems consonant to the Catholick Laws of Nature, that they should continue in that state of rest, till some force capable to over-power their resistance puts them out of it. And whatever may be said of the unloosable mobility of Atoms strictly so taken, yet that diverse parts of Matter may compose bodies that need no other Cement to unite them than the juxta-position which we here presuppose, and the resting together of their parts, whereby the Air and other fluid Bodies that might dissociate them are excluded, I have been inclined to think

think by what I have observed in grinding of Glasses: for sometimes the convex surface of one body being ground upon the concave surface of another, the two surfaces will happen to be so closely and exactly fitted to one another, (their immediate contact in all their parts, or at least in innumerable of them, hindering the intercurrence of the Air) that a man is not able without breaking one or both of them to pull them directly asunder; but if you will sever them, you must be fain to make one of them to slip along the surface of the other: which makes the Glass-Grinders often complain of the trouble they meet with in separating such bodies. Nay, if you lay two flat Glasses ground very true and well polished upon one another, so that their surfaces may almost every where touch each other, (to which it will be requisite to rub them a little one upon another, for the better exclusion of the Air) you may by lifting up the uppermost, lift up the lowermost (though perhaps, as we have often try'd, two or three times bigger) with it, as if the two Plates of Glass made but one body. Nay, we have divers times taken up four or five pieces of Glass at once, laid and prest thus one upon another, and might perchance have taken up a greater number, if we had had more of them at hand. And tryal has also informed us, that if you hold a Looking-Glass very level with the unfoliated side downward, and rub a little against it a piece of other very flat and very smooth Glass, you may easily by that way only, fasten them to one another; so that the lowermost Glass, though large, will hang between the uppermost and the ground, to the wonder of those that behold it, and can discern nor imagine nothing capable to keep it from falling: and by the same way (as we shall recite anon) we have often made one considerably thick piece of Marble take and hold up another, having purposely caused their flat surfaces to be carefully ground and polished,

lish'd, without which the Experiment will not succeed. Nor is it requisite that the glasses be flat to make them adhere very closely together, provided their immediate contact be made according to a large surface: for to what we have already mention'd concerning the cohering of convex and concave Bodies, we may adde, that having purposely appli'd a long glass-stopple of an almost conical figure, and well ground, to the mouth of a thick quart Bottle, whose neck was made long and of a figure convenient to receive the stopple, and ground within fit for it, we found that these two glass bodies touching one another in a multitude of parts, did adhere together so closely, that when the stopple was carefully put in, we could easily, and divers times one after another, lift up the bottle, though there was by our guess above a pound of Liquor in it. Unless we suppose, without much probability, that because 'tis found, that moving them to and fro upon one another, and pressing down the stopple, promotes their sticking, their adhesion may be in part ascrib'd either to some Elastical motion in the parts of the pressed glass, or to the exquisite adaptation of the almost numberless, though very small, asperities of the one, to the as numerous little cavities of the other; whereby the surfaces do lock in with one another, or are as it were clasp'd together. For as polish'd as the surfaces may appear to sense, we must not deny that there may be such inequalities in them, since being wont to be polish'd with Putty or some such powder, or heap of grating and irregularly shap'd Corpuscles, they must needs make store of little furrows, and ridges, and other Asperities on them. But to insist on these Conjectures were to digress.

Yet here we must not decline taking notice, that, at least here below, the sticking together of such bodies as are of sensible Bulk, and whose smooth surfaces immediately touch one another, may possibly not so much proceed

ceed from this, that their parts, as we formerly observ'd, are at rest among themselves, and by their immediate contact do make up as it were but one body; as from the pressure of the Atmosphere, proceeding partly from the weight of the ambient Air, (mixt with the Effluvioms of the terrestrial Globe) and partly from a kind of Spring, by vertue of which the Air continually presses upon the bodies contiguous to it, though through accustomance & negligence, & perhaps some other causes not here to be insisted on, we neither feel it in our own bodies, nor take notice of it in others. Now this pressure of the Air every way being suppos'd, I think the adhering of the smooth bodies we speak of (for we suppose them far greater than the particles of the Air) to one another may probably enough be ascrib'd to this, That when, for instance, the smooth surfaces of two pieces of Glass do so exquisitely touch one another, that none of the ambient Air is either intercepted or admitted between them, then the undermost glass will suffer no pressure on that side which touches the uppermost; the parts of the uppermost glass having no sensible spring in them (so that they can only Resist, but not Repell the other :) but that side of the undermost Glass which is expos'd to the Air will be press'd upon thereby; and there being, as we said, no Elastical pressure on the other side of the glass to balancethis, it is not to be wonder'd at that the inferiour glass should not fall off from the other, in regard the weight that would carry it downwards is much too small to overcome that force of the Air that thrusts it against the uppermost glass: As if one should with his hand thrust a plate of Iron broad-wise against the flat cieling of his Chamber, the Iron would not fall as long as the force of the hand perseveres to press against it. Nor is it material, that in our Case the pressure of the Atmosphere is suppos'd to force the lowermost glass upwards: For if we suppose the Air to consist of innume-

numerable little Springs (as it were) bearing upon and supporting one another, and whereof the lowermost are bent by the Weight of all that are incumbent on them, it will be easie to conceive that neer the surface of the Earth, (about which the Air must diffuse it self by reason of the Gravity of its small parts, and the Resistance of the Earth against their Descent) it may press almost equally every way, and by a kind of Recoyl (though not properly so call'd) from the Terrestrial Globe upwards, may strongly press any body upon which it can bear, against any other which has no such Elastical power to repel from it a body so press'd against it.

This Difficulty being thus dispatch'd, we shall proceed by two or three particulars to confirm our Conjecture at the Cause why smooth Bodies stick together upon bare juxta-position or contact. And first I observe, that if a piece of flat glass be, as we formerly mention'd, append- ed to a Looking-glass held with the unfoliated side down- wards, parallel to the Horizon, though the adhering glass will not drop down, yet it will very easily be mov'd any way along the level surface; and if by inclining the Look- ing-glass any way, you deprive it of its former Level, the smaller glass will easily slide downwards upon the sur- face of the greater. Of which the Reason seems to be, partly that the Gravity as such of the lower glass does not considerably resist the horizontal motion of it, but only the motion upwards, whereby it must recede from the Centre of heavy Bodies, as might, if need were, be pro- bably deduc'd from divers Instances obvious enough; and partly, or rather chiefly, that to the edges of the glass the Ambient Air is contiguous as well on the one side as on the other, and so the pressure of the Air being equal on every part of the edges, the gravity of the smaller glass is not hinder'd by the Air (which can as fast succeed on one side, as 'tis displaced on the other) from making it
slide

slide down the shelving surface of the greater Glass, where-
 as of the broad and flat sides of the lowermost glass the
 one is, as we said, press'd by the spring of the Air, whilst
 the other suffers no such pressure from the Looking-glass,
 to which it was apply'd. And so, if you take a small o-
 pen-mouth'd glass, and plunge it into a Vessel full of
 Quick-silver with the mouth upward, that the Quick-sil-
 ver may fill it without leaving any Air in it, and if then,
 whilst it is under the Quick-silver, you turn the mouth
 downwards, and so keeping it upright, lift it up till the
 mouth be almost come to the top of the Mercury; you
 shall perceive that the glass will remain almost full of
 Quick-silver in the Vessel: And this will continue so,
 though you incline the glass this way or that way, provi-
 ded you keep the mouth of it within the Mercury. And
 this Experiment, though more noble when try'd with
 Quick-silver, will succeed also when tryed, as it may
 more easily be, with water. Of which the Reason seems
 to be, that the glass hinders the Quick-silver in it from
 the pressure of the incumbent air, whereas the Quick-
 silver in the Vessel being expos'd to it, must by it necessa-
 rily be forced up against the surface of the inverted bot-
 tom of the glass, where it meets no Elastical power to re-
 pell it downwards. For, that it is not Natures Abhor-
 rency of a Vacuum, that keeps the Quick-silver from
 descending till some air can come to succeed in its room,
 the famous Experiment invented by *Torricellius*, and found
 true by many others, and our selves, touching the descent
 of Quick-silver in any Tube of above two foot and a
 half long, seems clearly to evince. And to confirm what
 we have said, and shew withall, that it is not so much the
 Contact of Bodies according to a large surface, as that
 Contact is considered in it self, as by reason of its being
 ordinarily requisite to the exclusion of Air, that at least
 here below keeps bodies from falling asunder; I shall
 relate

relate, that having by a certain Artifice out of a large glass (with a narrow mouth) caus'd a certain quantity of air to be suck'd, we found that by immediately applying a Book (which then chanc'd to lie at hand) to the Orifice of the Vessel, the Book was readily lifted up and sustain'd in the air as long as we pleas'd, though the surface of the suspended Body could be touch'd, as is evident, but by the Ring which incircl'd the Orifice of the Vessel, and though the weight taken up (besides that it was inconveniently shap'd for such a trial, which would probably have succeeded as well with a much greater weight,

** Much more considerable Instances of this nature may be met with in the Author's New Physico Mechanical Experiments.*

if we had had one fitly shap'd at hand) exceeded twenty Ounces*. Of which event the Cause seems plainly to be this, that by reason of the Exsuction of some air out of the glass, the Elastical power of the remaining Air was very much debilitated in comparison of the unweaken'd Pressure of the External air, which being able to press the Book against the Orifice of the Vessel with greater strength than the internal air can resist, thereby it comes to pass, that the whole Orifice of the Vessel, though there be but part of it of solid body, does yet on this occasion perform in some measure the part of an entire surface exactly smooth.

It may be consider'd also (to adde that upon the by) whether upon the Principle lately explicated may not in some measure depend the solidity of glass. For though its parts seem very little or not at all branched or interwoven one within another, and appear very smooth and slippery, yet since the fire that brought them to fusion, and consequently to be fluid, may well be suppos'd to have sub-divided and reduc'd them into very small Particles, and to have thereby assisted them to exclude the air from betwixt them, it may seem that it needs not much
be

be wonder'd at, if the immediate contact of such small and smooth Corpuscles suffice to make them hold together; for that their union is strict enough to keep out the air, may appear from this, that those that blow glasses, and those that distill in them, find not the air can traverse the pores even of heated glass; and as for any more subtil matter, we see by the free passage of Light and Heat, or, to speak more warily, of magnetical Effluvia through glass, without injuring its texture, that such matter but moderately mov'd will not hinder the little solid parts from cleaving together. And on this occasion it might be consider'd, how much the juxta-position of their Corpuscles crowded together by fusion may contribute to the consistence and brittleness of Salt-Petre, and diverse other bodies, which may from an incoherent powder be readily turn'd into one Mass; as also how far the sticking together (for I speak not of the figures compos'd by them) of the small parts of hanging drops of water, and such other Liquors as are not thought to consist of Corpuscles hooked or branch'd, may be ascrib'd to the contact of their small parts, and the exclusion of air. These, I say, and some other such things might be here consider'd, but that we are forbidden to examine them particularly, and especially what has been represented touching the solidity of glass, (which we suspect another cause may have a great Interest in) by our haste, which calls us to the remaining part of our Discourse.

Though then it be hence (to omit other proofs elsewhere mention'd) sufficiently manifest, that the Air has a spring, and that a strong one, yet there appears no great necessity of having recourse to it for the giving an Account why the two smooth glasses above mention'd were able to adhere so closely to each other: For a probable Reason of the same Phænomenon may be render'd by the pressure of the Air consider'd as a weight. And first,

we must recall to mind what we (a little above) said of the Recoiling, (or Rebouncing of the Pressure) of a Cylinder of Air from the Earth, to the suspended piece of glass, proceeding from this, that the fluid Air, which is not without some Gravity, being hinder'd by the resisting surface of the terrestrial Globe to fall lower, must diffuse itself, and consequently press as well upwards as any other way. Next, we may consider, that when the surfaces of two flat Bodies of any notable (and for example of equal) breadth do immediately touch each other, and lye both of them level with the Horizon, and one of them directly over the other; in this case, I say, since the Air cannot move in an instant from the edges to the middle of the two surfaces that lye upon each other, the lowermost cannot be drawn away downwards in a perpendicular line from the uppermost, but that by reason of the stiffness and contact of the two Bodies, it must necessarily happen that at the instant of their separation, should it be effected, the lowermost glass will be press'd upon by the whole (Crooked) Pillar of Air, suppos'd to reach from the top of the Atmosphere, and to have for Basis the superficies of the undermost glass. For at that instant, the Air having not time to get in between the two glasses, there is nothing between them during that instant to resist the pressure of that Air which bears against the lower superficies of that undermost glass, and consequently such a revulsion of the lower glass cannot be effected but by a weight or force capable to surmount the power of the weight of the abovemention'd Cylinder of the Atmosphere; and this, as I said, because that by reason of the sudden separation, the upper surface of the glass has not any air contiguous to it, which, were it there, would (according to the nature of Fluid and springy Bodies) press as much against the upper surface of the glass, as the Pillar of the Atmosphere against the lower, and consequently.

quently sustain that Endeavour of the Air against the lower side of the glass, which in our propos'd case must be surmounted by the weight or force employ'd to draw down the lower glass. And hence we may understand (to adde that upon the by) That it is not necessary that the contiguous surfaces of the two flat glasses we have been treating of, be parallel'd to the Horizon: For if you should hold them perpendicular to it, their division would not cease to be difficult, provided it were attempted to be made by suddenly pulling one of the broad surfaces from the other in a level line, and not by making one of the surfaces slide upon the other; for in the former case, the separation of the contiguous Bodies will be hinder'd by the weight or pressure of the lateral Air (if I may so speak) that bears against the broad sides of the glasses contiguous to it. But whereas in these cases we suppose the superficies of the two glasses to be so exactly flat and smooth, that no Air at all can come between them; Experience has inform'd us, that it is extremely difficult, if at all possible, to procure from our ordinary Tradesmen either Glasses or Marbles, so much as approaching such an Exquisiteness: For we could very hardly get either experienc'd Stone-cutters, or Persons skill'd at grinding of glasses, to make us a pair of round Marbles, though of an inch or two only in Diameter, that would for so much as two or three minutes hold up one another in the Air by contact, though they would easily enough take up each other, if the uppermost were drawn up nimbly, before the Air could have leisure to insinuate it self betwixt them.

But this notwithstanding, we endeavour'd by the following Expedient, not only to manifest that the Power or Pressure of the Air is in these Experiments very great, but also to make some Estimate (though but an imperfect one) how great that Power is.

Having then provided a pair of Marbles of an inch and half in Diameter, and as flat and smooth as we could get, and having consider'd, that as 'twas the getting in of the Air between them that (for the reason above declar'd) hinder'd them from sticking strongly together; so the Access afforded to the Air was for the most part due to that scarcely evitable roughness or inequality of their surfaces that remain'd in spite of the Polish: considering these things, I say, we suppos'd that the intrusion of the Air might be for some while prevented by wetting the surfaces to be joyn'd with pure Spirit of Wine; and that yet this Liquor, that seems the freest that we know of from tenacity, would not otherwise than by keeping out the air prove a Cement to fasten the stones together. But because the easie separation of such smooth Bodies as adhere but by contact does in great part (as we formerly noted) proceed from this, That whereas it is very difficult to hold such Bodies exactly level for any considerable space of time, and yet the least Inclination any way gives the lower Body opportunity to slide off; because of this, I say, we resolv'd in the first place to see what could be done by fastening to the upper Marble certain Wires and a Button, in such manner as that the lower Marble, when it was joyn'd, might freely fall directly down, but not slip much aside, being hinder'd by the Wire. And in pursuit of this we found, that not only the dry Marbles could be made to take up and hold up one another, but that once by drawing up the upper Marble nimbly, we could take up (but not keep up for any time) together with the lower Marble, a Scale, and in it a pound weight of 16 Ounces Troy.

After this we moisten'd the surfaces of the Marbles with such pure Alkalizate Spirit of Wine as we elsewhere teach to make, which was so thin and subtil, that not only we burn'd some of it before we would employ it
about

about this Experiment in a Silver Spoon, without leaving so much as any sign of Phlegm behind; but it would (in the open air) almost in a moment fly away from the surface of the Marble anointed with it, and leave it dry and glossy. The Marbles being skilfully wetted, and kept by the above-mention'd wires from slipping aside, we cast into a Scale fasten'd to the lower of them diverse weights at several times, and by nimbly pulling up the higher stone, try'd many times how much we could draw up with the lower, and did sometimes take up above an hundred Ounces, and once an hundred thirty two Ounces Troy, besides the Scale that contain'd them, and the Marble it self, the Diameter of whose smooth surface was by measure but about an inch and two thirds.

But here I must take notice, both in relation to this and the following Experiments to be set down concerning smooth Marbles, that we never yet found any sort of Experiments, wherein such slight variations of circumstances could so much defeat our Endeavours; which we therefore mention, that in case such Experiments be try'd again, it may be thought the less strange, if others be not able to do as much at the first and second, or perhaps the tenth or twentieth tryal, as we did after much Practice had made us expert in this nice Experiment, and suggested to us divers facilitating Circumstances, which could not here in few words be particularly set down.

And now, because we perceiv'd that the Spirit of Wine was too fugitive and subtil a Liquor for our purpose, we suppos'd that Oyl, as it would better fill up the little cavities of the Stones, so it would more exactly keep out the air, and less easily vanish into it. And accordingly, having moisten'd the surfaces with a due Proportion of good express'd Oyl of Sweet Almonds, and having carefully observ'd the other requisite Circumstances, we took up some drams above four hundred Ounces Troy hanging at the lower Marble. And

And that you may not suspect that it was by glewing the Marbles together, that the Oyl did enable them to make so much greater resistance against separation than the Spirit of Wine did; I shall adde, That in case the flat surfaces of the joyn'd Stones were held not parallel, but perpendicular to the Horizon, that so the air might (as we formerly also observ'd) immediately succeed as the looser Marble should slide off, the weight of some Ounces was now and then requisite to draw down the Marbles when they had nothing but Spirit of Wine between them, whereas they would easily enough slide off from one another when they were cemented together with Oyl: perhaps because that the Spirit of Wine by reason of the smallness and penetrancy of its parts, and because of its fugitive nature, did not so well fill up the little pores and furrows of the surfaces of the Marbles; whereby the little protuberances getting into those little cavities, might more resist the sliding of the Marbles upon one anothers surfaces, whose texture is better fitted to make their surfaces smooth and slippery.

And to shew that the resistance of such contiguous Marbles to a violent separation is greater in those which being broader are press'd against or resisted by a proportionably bigger (though not a longer) Pillar of the Atmosphere; We caused two Marbles to be made, whose Diameter was three inches or a very little more: and having after the abovemention'd manner employ'd Spirit of Wine to keep out the air from between them, we did after some tryals, with the uppermost of them take up the lowermost, and with it four hundred sixty eight, or four hundred and seventy Ounces. But making use of Oyl of Almonds instead of Spirit of Wine, we did with our own hands draw up twice, one time after another, with the undermost Stone, a much greater weight, namely, eighty four pound or 1344 Ounces (Troy weight) notwithstanding which
weight

weight we manifestly perceiv'd the Marble at which it hung to stick strongly to the other.

And here again we will take notice, that the interpos'd Oyl was so far from being able as a Cement, rather than by keeping out the air, to make these Marbles stick so close together, that, whereas Bodies glew'd or cemented together are wont to make an almost equal resistance to their being separated, in what Posture soever you place them, I made our Marbles, even when we try'd this last Experiment, very freely slide upon one another, by impelling the uppermost to the right hand or to the left, with my Finger or my Thumb: (whereof the reason is intimated above, where we mention'd almost the like case in Glasses) and having sometime before taken up a weight which we conjectur'd to be not much inferiour to that last named, we presently for tryal sake held the Marbles with their edges downwards, and found that those that in an Horizontal Position could not be drawn asunder by so great a weight, did in another posture presently fall asunder by their own weight, which made one readily slide off from the other to the ground. Now although we have confess'd, that this way of measuring the force of the Air is not Accurate; yet we hope it will not be thought Useless, since (not to mention that by thus breaking the Ice, we may make way for the happier Enquiries of others) it not only shows us that this pressing or sustaining force of the air, as unheeded as it is wont to be, is very *Great*, but it may also assist us to conjecture *how Great* it is, which though we cannot hereby determine precisely and with certainty, yet we may estimate it with much less uncertainty than otherwise we could.

I know that the Peripateticks, and the generality of the School Philosophers, will confidently ascribe the sticking of the Marbles, not to the cause we have assign'd, but to Natures Abhorrency and fear of a Vacuum. But not

to engage our selves now in a Disquisition that when we discours'd of Fluidity we did (for the Reasons there express'd) decline to meddle with; We will, without disputing whether or no Nature either can at all admit, or do abhorre a Vacuum, content our selves to confirm the Explication given of this Phænomenon by these two Considerations. The one, That if Nature did so violently oppose a Vacuum as is pretended, it is not likely that any force whatsoever that we could employ would be capable to produce one; whereas in our case we find, that a little more weight added to the lower of the Marbles, is able to surmount their Reluctancy to separation, notwithstanding the suppos'd danger of thereby introducing a Vacuum. And my next Consideration is, that (according to what we have hitherto deliver'd) without having recourse to any such disputable Principle, a fair account may be given of the propos'd Phænomenon; by the pressure or weight of the Air. And that what we have said concerning the latter of these may be entertain'd with the less difficulty, let us suppose, that when the Marbles stick well together, the lowermost of them, or the appendant weight were fasten'd to the ground: For in this case there appears no reason to believe that their power to resist separation would be less than it was before. And yet it seemsevident, that the uppermost Marble would not be perpendicularly pull'd up but by such a force as were at least (I say at least) able to lift up a weight equal to that of the last mention'd Marble, and of a Pillar of Air having the Stone for its Base, and reaching to the top of the Atmosphere; since at the instant of Revulsion, before the Air can get in, and spread it self between the Stones, there is not for ought appears any such Body under the upper Marble, as can help the hand to sustain the weight both of that Marble and the incumbent Cylinder of the Atmosphere, which then gravitates upon it, and consequently

quently upon the hand ; because there is no Air, nor other equivalent Body underneath it, to sustain its part of the weight, as the lower Air is wont to do in reference to the heavy Bodies that lean on it, and to the weight of the incumbent Air. And therefore we need not much marvel, if when only a less weight than that of the foremention'd Pillar of the Atmosphere hangs at the lower Marble, it should be capable of being drawn up by the uppermost, rather than suffer a divulsion from it. As we see that when two Bodies being fasten'd together, are endeavour'd to be drawn asunder by forces or weights not able to separate them, they will usually both of them move that way, towards which either of them is the most strongly drawn. On which occasion, I remember what I have sometimes observ'd in one of the wayes of trying the strength of Load-stones : For if the Load-stone be able to take up more than its own weight, you may as well lift up the Load-stone by a Knife, as the Knife by the Load-stone. And though one accusom'd to judge only by his Eyes, would have imagin'd, that when I held the great weights formerly mention'd suspended in the Air, there was no strong endeavour to pull up the upper Marble from the lower, because my hand being for a while held steady, seem'd to be at rest ; yet he will easily be invited to suspect that in such a thought there may be a great mistake, who shall consider, that neither did the weight sensibly appear to pull the lower Marble downwards, though my hand assur'd me that the weight had not lost its Gravitation. And if I shall adde, that once, when the weight after having been lifted up into the Air, was casually so loosen'd from the upper Marble, as suddenly to drop down, my hand, unawares to me, was by the force of that Endeavour it just before employ'd to sustain the fallen weight, carried up with such violence, that I very sensibly bruis'd it by the stroak it gave against the face of a By-stander,

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stander, who chanc'd out of curiosity to hold his Head over the Marbles.

And here it will not be impertinent to bring in an Experiment that I once devis'd, not only for other uses, but to illustrate the subject we have been hitherto treating of. The Tryal I lately found registred among my *Adversaria*, in these Termes. A Brass Valve of about an Inch Diameter was with Cement well fastned to the shorter Leg (which was but of very few Inches in Length) of a long Glass Syphon left open at the end of the other Leg. This Valve being let down to the Bottom of a tall Glass Body full of water, so that 'twas (if I much mis-remember not) between a Foot and half a yard beneath the surface of the water, when there was let in as much water into the Pipe, as reach'd in that as high as the surface of the External Water in the Tall Cucurbite: Then about an Ounce weight was put into the opposite Scale of a Ballance, to the neighbouring Scale whereof one end of a string was tyed, whose other end was fastned to the said Valve, whose parts would be thereby drawn asunder. But when the water was empty'd out of the Pipe, and the Valve was let down to the former depth, there was requisite about 5 Ounces, that is 4 Ounces more than formerly, to disjoin the parts of the Valve, and let the water get in between. And when (the Syphon being freed from water) the Valve was lifted higher and higher, together with the Pipe, there needed less and less weight to make a Disjunction; two Ounces of Additional weight (to the one Ounce requisite to counterpoize the Cover of the Valve it self) on the water sufficing to lift up the Cover, when the Valve was held about half way, between its Lower station, and the Top of the water; a single Ounce sufficing afterwards, and half an Ounce of Additional weight proving enough to disjoin the parts when the Valve was held but a little beneath the surface of the Liquor.

This relation of an Experiment which I afterwards show'd to many *Virtuosi*, will perhaps seem somewhat dark to you without a Scheme; but if you consider it attentively enough to apprehend it thoroughly, I presume it will show you, that whether or no there be upon any other score a repugnancy to the separation of smooth Bodies join'd by immediate contact; yet certainly there may be a great Repugnancy upon the bare Account of the Gravity of the medium, wherein the Divulsion is attempted. For in our case the *Fuga Vacui*, if there be any, ought to resist the separation of the Parts of a Valve still kept under water, as much near the Top of the water, as at the Bottom. And therefore the great difference found in that resistance at those different places, may be attributed to the Pressure of the Ambient water, that thrust them together. And though it be true that Air is an Exceeding Light Body in comparison of water; yet in divers Tryals I have found the Disproportion in Gravity of those two Fluids not to exceed that of a 1000. to 1. So that considering how many miles, not to say scores of miles, the Air may reach upwards, there seems no absurdity at all to suppose that the bare Pressure of it against the Marbles formerly mention'd, may keep them as coherent as we found them to be.

But since this I have been able to make an Experiment, that does sufficiently confirm the former Doctrine. For having suspended the two coherent Marbles in a Capacious Glass, whence by a certain contrivance, the Air could little by little be drawn out, we found as we expected, that whilst there remain'd any considerable quantity of Air in the Glass, the lower Marble continued to stick to the other, the Pressure of the remaining Air, though but weak, being yet sufficient for the sustentation of the lower Marble, which it was not after the Air was further withdrawn. And if when the Disjunction was made, the upper Marble were by another contrivance let down upon the lower, so as to touch it as before; though whilst the External Air was kept out of the Glass, the upper Marble might easily be rais'd without taking up the lower with it; yet when the outward Air was let in, the Marbles were press'd together, and became again strongly coherent.

But it is now high time to look back to that part of our Discourse, which the consideration of our Marbles has so long intic'd us from directly prosecuting. Although then it may from the past Discourse be conceiv'd, that in Bodies of sensible bulk, whose smooth surfaces touch one another, the force of the Air does mainly make them cohere; yet it seems that generally in Bodies (whether greater or smaller) it is a sufficient cause of Cohesion that the parts of the Body are at rest by one another, though perhaps the entire Concretion be remov'd from place to place. For Bodies of sensible bulk being either fluid or consistent, and it being (as above we have taught) the chief requisite of a fluid Body that its small parts be in motion, there seems not any thing necessary to keep a Body from being fluid, and consequently to keep it a firm Body, but that its contiguous parts be in a state of rest.

I know that almost all Philosophers both ancient and modern, require something else than the Rest of the parts (of which scarce any of them takes the least notice as of a thing conducive to Firmness) to the keeping together the parts of a dry and stable Body. But although to engage very far in such a Metaphysical and nice Speculation were unfit for me, (at least at present, when I am but to endeavour to explicate Fluidity and Firmness in the sensible Bodies we converse with;) yet we dare not quite skip it over, lest we be accus'd of Overseeing it. The greater number of Contemplators ascribe the effect under Consideration to a certain substantial form, to which they assign, among other Offices, that of keeping all the parts united into one Body. But what this form is, and by what means it unites the parts so strongly in a Diamond or a Ruby, &c. and so loosely in Tallow, Camphire, or the like slight concretions, and how the substantial form continuing the same in Water and Ice, the same matter may easily and frequently become by turns a hard and a fluid Body;

Body; how these, I say, and diverse others things are effected by the forms of solid Bodies, is to me, I confess, at least as difficult to conceive, as to imagine without it a cause of Cohesion in the parts of a dry Body.

Other Learned Men there are among the modern Naturalists who have recourse some of them to a Spirit, which penetrating and fastening to each other all substances corporeal, unites them into one World; but others fancy rather a certain Cement or Glue, whereby they conceive the parts of Bodies to be made as it were to stick to each other. But as for this last Hypothesis, it would be consider'd, that though Glue is made use of to joyn together Bodies of sensible bulk, yet Glue it self being a Body that is so too, it must also it self consist of lesser parts sticking to one another; which allows me to demand the cause of the mutual Coherence of those parts. And if it be answer'd, that they likewise stick together by the Intervention of some more subtil Glue, I shall again represent that this Glue also must consist of corporeal parts; and therefore I shall further demand how these also stick together: and if the like Answer be again made me, I shall still renew the like Demand, till at length the Answerer be reduc'd to confess, that parts of Matter so very small cannot be reasonably suppos'd to be kept together by a Cement. And if the Corpuscles that make up the finest Glue imaginable are not kept together by a Cement, we may be allow'd to ascribe their Adhesion to the immediate Contact and Rest of the component parts, (which is a cause intelligible, and at least probable) till some other sufficient cause be assign'd, which I do not take that to be which is taught by the Patrons of the first Hypothesis lately mention'd, concerning a Spirit diffus'd through the mass of matter. For not to mention that the Agility of a Spirit seems not so proper a Qualification for that which is to fasten Bodies together, we may consider, that this substance

stance which is called a Spirit is indeed but a subtil Body. And why therefore may not the minute parts of other Bodies, if they be conveniently shap'd for Adhesion, stick to one another, as well as stick to this Spirit? And I should here also demand, how the parts of this Spirit are kept fast to one another. If any should answer, That this Spirit consists of parts which are inseparable, and yet perhaps of a hooked shape, which fits them to fasten themselves to the Bodies they take hold of, and thereby those to one another; this would be to propose such a new Notion of the diffus'd Spirit, as I know not whether those whose Opinion I have been examining did ever dream of, or would be content to adopt: and sure according to this Hypothesis there must be a wonderful plenty of these little Spirits in the grossest Bodies; and Ice for example, which is thought so destitute of Spirits, must be well-nigh half made up of them: For these little spirituous parts can fasten no parts of other Bodies together but those they touch, since otherwise the parts of other sorts of matter, if but contiguous, might cohere without these, which is against the Hypothesis. And since each of these small spirituous Corpuscles, if I may so call them, being really a natural Body, and by consequent necessarily divisible, at least by Thought, into parts, I shall ask the Proposers of this new Notion of Spirit, upon what Account this Corpuscle can be indivisible; I mean, what it is, if it be not Rest and immediate Contact, that hinders but that the parts (or designable parcel of corporeal substance) which are divisible by thought, should be alwayes kept together, and never be actually divided. I am not averse indeed from granting, that they may almost alwayes escape Dissolution: but I am apt to suspect that may be, because that by reason of the extreme smallness, and the Rest and strict Contact of their parts, they can scarce ever meet with an Agent minute and swiftly enough mov'd;

to be able to shatter them or dissociate the combin'd parts. For to say, that 'tis the Nature of every such Corpuscle to be indivisible, is but to give me cause to demand how that appears: For so important an Assertion needs more than a bare Affirmation for proof.

And if two of these Corpuscles that are presum'd to be indivisible should, being smooth and of the same figure, (as for Example Cubical) happen to lye upon one another, and a third should likewise chance to be fitly plac'd upon the uppermost of the two, what should hinder but that this Aggregate may by the violent knock of some other Corpuscles be broken in the midst of the whole Concretion, and consequently in the middlemost body? For suppose them as Adamantine as you please, yet since Corpuscles as hard as they are, can be made very violently to knock against them; why may not these grate or break the middlemost Corpuscles, or any of the others? as we see that Diamonds themselves may be reduc'd to powder by other Diamonds, though not (as Artificers vers'd in the Trade inform me) by Attrition with any other stone. To prove that the Cohesion of the middlemost of the three lately-mention'd small Deyes with the other two, the one above it, the other below, is not so strong as that of the parts of that middlemost Corpuscle, notwithstanding that the contact between each two adjoining Bodies is suppos'd to be full, (for so it must be in such Bodies, though not always in others visibly greater, in which some subtiler substance may be suppos'd to come in part between them;) to prove this, I say, there must be assign'd some better cause of the Cohesion of the matter in one part of the propos'd Body than in the other. And it cannot with probability be pretended, that a Corpuscle presum'd incapable to be divided should consist of hooked parts: and if that should be pretended, yet ev'n these hooks also being true Bodies, the Question would recur concerning
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Them, and be still renewable *in infinitum*. If it should be said that these minute Bodies are indissoluble, because it is their nature to be so; that would not be to render a Reason of the thing propos'd, but in effect to decline rendring any. And though I know that in every Hypothesis about the principles of things, something is allow'd to be assum'd, as not being to be explain'd or prov'd by any thing more primary than it self; yet I know not whether this excuse be proper in our Case, wherein it seems that the entireness and permanency of any parcel of matter, how minute soever, may be probably enough deduc'd from the immediate Contact, the Rest and the extreme littleness of its designable parts. And if for a last refuge it should be said, that the designable parts of these Corpuscles are therefore unseparable, because there is no vacuity at all intercepted between them; besides that this is contrary to the supposition formerly made; for such extremely minute Deyes as we imagin'd to be one upon another, having their surfaces according to our Postulatum, flat, smooth, and exquisitely congruous, could no more than the parts of either of the three Corpuscles have any vacuity intercepted between them: besides this, I say, this is both to suppose a Vacuum in all divisible Bodies, and that too as the cause of their being such, and to decline the former Hypothesis touching the use of this Spirit, and take Sanctuary among the Atomists, to whose opinion about the account upon which those Bodies they call Atoms are not dissipated, although some of the Considerations we have alledg'd against the newly examin'd opinion may in part be appli'd; yet diverse of their other opinions do so fairly comport with the generality of our Experiments in these Notes touching Fluidity and Firmness, that I am willing to decline clashing with them, by not pursuing now any further a Disquisition, which, as I said a while ago, is not necessary to my present design: espe.

cially, since the dim and bounded Intellect of man seldom prosperously adventures to be Dogmatical about things that approach to Infinite, whether in vastness or littleness. Nor indeed would I have that look'd upon as a resolute Declaration of what I think of so abstruse a Subject, which I have rather propos'd to avoid saying nothing, where I suppos'd it expected I should say something.

The other thing then, which in our Description of a firm Body we mention'd as capable to make it so, is the texture of the parts (whether homogeneous or not) that constitute it; and though the Juxta-position and Rest of these parts may possibly alone suffice to make the Body stable; yet this Texture seems to be the most usual cause of stability, and sometimes also it may superadde a degree of that quality to that which bodies may have upon the former account only. For, though whilst the parts of the Body are actually at rest it cannot be fluid, yet those parts, if they cohere to one another but by rest only, may *Cæteris paribus* be much more easily dissociated and put into motion by any external Body actually mov'd, than they could be if they were by little hooks and eyes, or other kind of fastenings intangl'd in one another; it being often necessary in this case violently to break off these fastenings before the little bodies fasten'd together by them can be disjoyn'd, and put into such a separate motion as is requisite to the constituting of a fluid Body.

We formerly made use of that familiar substance, the white of an Egge, to illustrate the nature of Fluidity: Let us now try whether it will also assist us in our enquiry after the causes of stability. When an Egge is made hard by boyling, since whether we suppose this Induration to be effected by bare motion or impulse, or else by the insinuation of fiery Corpuscles, since I say there is nothing that appears to get in at the shell, unless perhaps some calorifick Atoms, and perchance too some little particles of the fluid water it is

boyled in, 'tis not easie to discover from whence else this change of consistency proceeds, than from a change made in the texture of the parts whereby they are connected and dispos'd after a new manner, fit to make them reciprocally hinder the freedom of each others motions. But if instead of hardning the whites of Eggs by the heat of the fire, you beat them very well into froth, you may perceive that froth to emulate the nature of a stable Body: for not only you may raise it up to a pretty height, and make it retain a sharp top almost like a Pyramide; but I remember I have for curiosity sake made with a little care a long and proportionably thick Body of these bubbles hang down from my finger without falling, like an ice-icle from one of the Reeds of a thatch'd House, and yet in this there appears not any alteration to be made in the fluid Body, save a meer Mechanical change of the disposition of its parts: which may be confirmed by water beaten into froth, for there the heaped bubbles will quickly subside and fall back into water of the very same consistence it was of before.

Now there may be several things whereby a body may be put into such a texture as is convenient to make it firm or stable. And of these, before we consider of them particularly, it will be fit to take notice in general, that for the most part 'tis not from any of them Single, but from two or more of them Concurring, that the Stability of Concretions proceeds.

The first and chiefest of these seems to be the fitness of the shapes of the component particles to fasten to each other; as if some were figur'd like the handles of Buckets, and others like the hooks that are wont to be employ'd to draw them up out of the Well, or some like buttons, others like loops, some like male, others like female screws (as Mechanicians speak) or as if many together were so variously branch'd, that their parts may be so interwoven one within another, as not to be easily separable (as we often see in

a well-made dry hedge, of which if a man go to pull away one bough, he shall often be unable to do it without pulling away with it diverse others whose slender twigs will be intangled with it.) An eminent example of the power of the bare Texture of many small Bodies (even such as each of them apart is not perhaps extraordinarily shap'd for such a purpose) to make a stable one, is afforded us by Ropes and Cables; where only by twisting together and wreathing the slender and flexible threds the Cable is made up of, they are so well as it were wedg'd in between and fasten'd to one another, that they constitute a Body not to be broken by the weight of an Iron Anchor, nor perhaps by the force of a Ship violently driven on by the fury of the Winds and Waves. This figuration of the Corpuscles that make up consistent Bodies, seems to have been the chief if not only cause of their consistence in the Judgment of the antient Atomists, this being the account that is given of it by *Lucretius*.

*Denique quæ nobis durata ac spissa videntur,
Hæc magis hamatis inter sese esse necess' est,
Et quasi ramosis alte compacta teneri.
In quo jam genere imprimis adamantina saxa,
Prima acie constant, ictus contemnere sueta,
Et validi salices, ac duri robora ferri,
Æraque quæ claustris restantia vociferantur.*

And indeed, so innumerable may be the correspondent figures which are fit to fasten bodies to one another, that it is very possible that two bodies, whereof each a part is fluid, may upon their Conjunction immediately intangle their parts in one another, and thereupon acquire such a new texture, that their parts cannot as formerly dissociate themselves at pleasure, and move along one anothers surfaces, nor consequently flow after the manner of Liquors, but are

so connected or intangled, that the motion of one of them will be resisted by many, and so the whole Body will become firm or stable. Something like this may be seen in the Experiment mention'd by our Author, where he teaches that the distill'd Liquor of Nitre, and that made *per Deliquium* out of fix'd Nitre, will presently upon their mixture in part concoagulate into saline and consequently stable Bodies : but this seeming only a re-union of the saline particles that did, though invisibly, swim up and down in the aqueous parts of the mingled Liquor, which after this separation remains both more copious than the saline parts, and as fluid as before, we will adde a noble instance (mention'd to another Purpose by *Lully* and *Hartman*) to declare how much the firmness of bodies depends upon their texture. If you take then the Alchool or highly-rectify'd Spirit of Wine, and exquisitely deflegm'd Spirit of Urine, and mix them in a due proportion (as I remember the last time I made the Experiment I took about equal parts by guess, though two of the former to but one of the latter, if This be excellent, be a better Proportion ;) you may in and about a minute of an hour turn these two fluid Liquors into a constant Body ; and I confess it was not without pleasure, that I have immediately upon the shaking of these two Liquors seen them shoot into the likeness of Snow, and acquire such a consistence, that I could without spilling the mixture turn the vessel that contain'd it upside down.

But I dare not expect to have this Experiment believ'd ev'n by most of them that shall try it, Experience having taught me, that it will not succeed, unless the Spirits of Urine and of Wine be both of them more exactly deflegm'd than is usual even among Chymists. Yet so much more does this coagulation seem to depend upon the Salt of Urine as of such a texture, than barely as Urinous, that we will add that, As the spirit of fermented Urine is not (whatsoever some eminent Chymists may think or say) so indispensably requisite,

quisite, but that my curiosity leading me to try whether other Liquors, which I suppos'd to be of a resembling nature, might not serve the turn, I found that sufficiently-rectified Spirit of Harts-horn (to mention that alone here) may be made to supply its place: So I endeavour'd to make it probable by this, That having try'd a certain method (though that may seem strange to most Chymists) of so ordering Urine, that without staying at all to ferment or putrifie it either forty days or half so many hours, I can make the volatile or saline Spirit ascend first in distillation, though I use but some such gentle heat as that of a Bath: Having, I say, by this means distill'd a very strong Spirit of unfermented Urine, and rectify'd it too, I found, as I expected, that I could not by any means make it coagulate with Spirit of Wine, which seem'd to proceed from the differing texture of this Spirit from that of Fermented, or rather Putrify'd, Urine, since I had added nothing to the fresh Urine I distill'd, but what was extreamly fix'd and belonging (as Chymists speak) to the mineral Kingdom. Whereupon having had the curiosity to enquire of some of my Chymical acquaintances, I found that they complain'd that they had not been able to coagulate Spirit of Wine with the saline Spirit made of meer Urine, without any addition at all, when they distill'd that Urine without a previous putrefaction (which is not wont to be perfected under six weeks or thereabouts.) But to return to our Coagulum we will annex, That this is further remarkable in this Experiment, that this white coagulated substance being put into a glass vessel exactly stop't and kept in a gentle heat (which yet it self is not perhaps necessary, though expedient) for some weeks or months, will at least for the greatest part by much (for I have not yet totally seen it do so) resume the form of a limpid Liquor; as if either all the crooked particles that connected the small coalitions of the Vinous and Urinous

This Way the Author afterwards Published in another Book.

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Corpuscles to one another, were by the motion they were put into by the external heat one after another broken off, or else the same little concretions (for the *Menstruum* seems to consist chiefly of them, being able to perform other matters than either of the single Liquors whereof 'tis constituted) either assisted by outward warmth, or inabled thereto by some other cause of mobility, did after many and various attempts to clear themselves of each other, little by little so unbend or break off the crooked particles that intangled them, as at length to extricate themselves, and become capable of freely shifting places among themselves, and so of the form of a Liquor.

And here I shall adde a couple of Experiments for the sake of their affinity with some parts of the newly recited Experiments about the Vino-Urinous Coagulate.

And first it seem'd to me worth trying, whether some Acid Salts being duly order'd would not concoagulate with Spirit of Wine, as well as with Urinous Salts; and having for a while digested together in a convenient proportion pure Saccharum Saturni, made with Spirit of Vinegar, and rectify'd Spirit of Wine, I found the mixtures so chang'd in point of Consistence, that upon inclining the Glass which contained it, none of it would run down the Sides. But this Experiment did not afterwards seem to me either easie or consistent, nor is it more then one of the ways, and I doubt none of the best, of attempting what we have propos'd.

The other Experiment I promis'd you, relates to the resolution of the Coagulum of Spirit of Wine (which to be performed by digestion requires a very long time) And I wish I had not some reasons to hinder me from communicating to you the way of making of it at present; it being an Experiment that seems somewhat strange in its kind; but that part of it which is directly pertinent to our present Argument, you will, I trust, believe upon my Relation,
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which is, that by the addition of nothing but of a very fixt and very dry Body (insomuch that 'twill not yield any thing by the common way of distillation even in a naked fire) by the sole addition, I say, of this dry Body, the newly mention'd Coagulum, which is also a consistent Body, may in a few hours be brought into a permanent Liquor (quite distinct from the dry Body) which when Experience first recommended this way to me, was of too subtle and penetrating a Nature, not to make me expect from it considerable Effects, both in Chymistry and Physick, of which imitation you may be pleas'd to take notice.

But to return to what I was about to subjoyn after the mention of our Coagulum, as that is an Example of firmness produced by Texture; I will here, because it is not easie to procure Spirits pure enough to make such a Concretion as that. I will here, I say, set down another way of speedily hardning one fluid Body by another; for if you take the white of an Egge, and beat it till it become thin, and then shake well into it about half its quantity (perhaps much less might serve the turn) of right Spirit of Salt, you shall have in a few minutes the mixture so coagulated, that I remember when we turn'd the glass wherein we made it upside down, not a drop of Liquor did run out, though some hours after we obtain'd a little by breaking the crudled matter. And another Experiment much of the nature of this is said to be delivered by Sir *Francis Bacon*, who teaches to coagulate whites of Eggs with Spirit of Wine: and indeed, if you observe a circumstance (unmention'd, that I hear of, by him) which is the shaking of the two Bodies well together, and if your Spirit of Wine be good, the Experiment will succeed very well, insomuch that I remember I have made this way a Coagulum, from which no Liquor would drop down in about a minute of an hour. But whereas this great Naturalist conceives this hardning of the Egg's white to be perform'd by the heat of the Spirit
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of Wine, I shall willingly confess he has assign'd the cause ingeniously, but must doubt whether he have done it truly: for there are diverse things that seem to argue Spirit of Wine, as inflammable as it is, to abound with a piercing Salt, and that such saline Corpuscles may suffice to crudle whites of Eggs, our freshly-mention'd Experiment of crudling the white of an Egge with Spirit of Salt does sufficiently declare; and not only we have perform'd the like effect with some other Acid Spirits, and particularly that call'd Oyl of Vitriol, but it may be produc'd, though more slowly, ev'n by a crude Salt; for by long beating the white of an Egge with a lump of Allum, you may bring it for the most part into white cruds. So that if we will allow the coagulation we treat of to be performed by the Spirit of Wine as hot, it seems that that heat must be only such as may be ascrib'd to the active particles of saline bodies, which yet are commonly accounted rather cold than hot. But because I somewhat doubt how justly they are reputed so, I will adde, that I did purposely for tryals sake, take the Serum or Whey that is wont to swim upon mans Blood after it is cold and settled, and indeavour'd in vain to coagulate it with such Spirit of Wine as would coagulate Eggs, and yet this whey will at least as soon as (if not much sooner than) whites of Eggs coagulate over a gentle heat of Embers; which makes it doubtful, whether the effect proceed not from the greater correspondency in texture of the Spirit of Wine with one of the Liquors than with the other, rather than from the heat ascrib'd to it, which did not at all coagulate the whey.

But although we have mention'd some Examples to shew that two fluid Bodies may be associated into a consistent one; yet we want not an Experiment to make it appear, that likewise by the change of Texture a fluid Body may be divided into two consistent ones. This Experiment which we have partly taken notice of before (treating of Fluidity)

Fluidity) is, that having for tryals sake by convenient degrees of fire distill'd over a due proportion of the more volatile parts of fallet Oyl, neither the Liquor that came over, nor the substance that remain'd behind in the Retort was fluid, though the Oyl that yielded them had been so.

But when I put to the Oyl before Distillation a convenient quantity of common Salt, and one or two other things, that were fit to change the Texture of the branch'd or hookt Corpuscles whereof it consisted; I could then obtain an Oyl of common Oyl, that both dropt into the Receiver in the form of a Liquor, and continued a Fluid Body; which may probably be of good use to Surgeons, Varnishers, and Men of some other Professions.

And to make it the more likely, that by Additaments of some such nature as that newly mention'd, some grosser and cloggy parts are retain'd, or else much subtiliz'd and otherwise altered. I shall adde that prosecuting a hint I happen'd to meet with in the discourse of a wandring Chymist, I practis'd a way so to defecate the dark and muddy Oyl of Amber drawn *per se*, that a pretty proportion of it would come over so transparent and finely colour'd, that the Experiment did not a little please those I shew'd it to. And if it do not appear upon tryal, that this way of preparing Oyl of Amber does by detaining some parts, which though more gross then the rest, may yet be no useles one; impair the Remedy, and that it does not, upon some other score infringe the medicinal vertue of the Oyl, the Experiment will not be unuseful. For the Liquor that is thus prepared is not only very diaphanous and well colour'd, but so pure and subtle that 'twill swim, not only upon Water, but upon Spirit of Wine it self. And 'twill be no despicable thing, it by extending or further applying this Experiment to other indispos'd Bodys, many Emphyreumatical Oyls distill'd by
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strong fires in Retorts, can be brought to emulate essential Oyls (as Chymists call them) drawn in Limbicks, as to clearness and lightness.

The additament I last thought fit to make use of for purifying Oyl of Amber was briefly this. *Rx.* Two Pound or somewhat less of good Brandy, One Pound of good Sea-Salt, and half a Pound of the Oyl to be subtiliz'd, mix and distil them together.

Upon the mencion I made above of the white Coagulum of the Spirits of Wine and Urine, I remember what I have sometimes observ'd in the essential Oyl of Anniseeds (as Chymists speak) distill'd with store of water in a Limbick and Refrigeratory, namely, that in the heat of Summer it would remain a perfect Liquor like other Chymical Oyls; but during the cold of the Winter, though they, notwithstanding that season, continued fluid as before, the Oyl of Anniseeds would coagulate into a Body, though not of an uniform Texture to the Eye like Butter (but rather almost like Camphire) yet like it white and consistent, not without some kind or degree of Brittleness.

And on this occasion I will here insert an Experiment which should have been set down in that part of the former History of Fluidity, where I mention, that the small parts of a Body may be sufficiently agitated to constitute a Liquor by the Air or other Agents not sensibly hot themselves. The Experiment take thus. Casting by chance my Eyes in the Winter time upon a glass of Oyl of Anniseeds which stood coagulated by the cold of the season, I presently bethought my self of making a Liquor (whose process belongs to another Treatise) of which as soon as I had prepar'd it I made this Tryal. I melted with a gentle heat the congeled Oyl of Anniseeds to make it flow, and then cover'd part of it in another glass with a Mixture I had provided: and having let them both rest in the window,

I found, that the meer Oyl being fully refrigerated again, coagulated as before; but that which was cover'd with the other Liquor continu'd fluid both day and night, and in several changes of weather, and does still remain at the bottom of the *Menstruum* a clear Oyl distinct from it, though I have purposely shaken them together to confound them.

And because, *Pyrophilus*, I have not discover'd to you the *Menstruum* I made use of, I will here present you with a Succedaneous Experiment made with a common Liquor. I took then good clear Venetian Turpentine, and having slowly evaporated about a fourth or fifth part of it, till the remaining substance being suffer'd to cool would afford me a coherent Body, (or a fine Colophony) I caus'd some of this transparent and very brittle Gum (of which I have else where taught you some uses) to be reduc'd to fine powder: of which I put into pure Spirit of Wine a greater proportion, then I judg'd the Liquor was capable of dissolving, to the end that when the Spirit had taken up as much of the Powder as it could, there might remain at the bottom a pretty quantity of our Colophony. On which, though the *Menstruum* (being already glutted) could not act powerfully enough to dissolve it, yet it might give the matter (which it had already so far softened, as to reduce it into a coherent mass) agitation enough to emulate a fluid (though somewhat viscous) Body. And accordingly I obtain'd a sluggish Liquor, which continued fluid, as long as I pleas'd to continue the *Menstruum* upon it. The like Experiment I try'd with clarify'd Rosin, and with fine Colophony, though but bought at the Shops; and although the Tryal sometimes succeeded not ill, yet I found not the success constant and uniform, whether because the Bodys to be dissolv'd were not defecated and pure enough, or that I did not hit upon the best proportion between the Solvent and them. But this circumstance I

shall not omit, that when the glutinous Liquor was separated from the *Menstruum*, it would by degrees, though but slowly, harden in the Air. The Application of which property, for the preservation of small and very tender Bodies, I shall not here more expressly hint then by having barely nam'd it. I had forgot to adde, that whilst the substance continu'd fluid, I could shake it, (as I lately told you I could the Oyl of Anniseeds) with the supernatant *Menstruum*, without making between them any true or lasting Union.

Which last circumstance brings into my mind another Experiment that I likewise forgot to adde to that part of the former History of Fluidity, where I take notice, that the particular Textures of fluid Bodies maybe reckon'd among the chief causes of their being dispos'd, or indispos'd to mingle with one another. For partly to confirm this Conjecture, and partly to manifest that 'tis not universally true which Chymists are wont to think, that Acid Salts and Oyls will not incorporate or mingle; I took an arbitrary quantity (and, as I remember equal weight) of common Oyl of Vitriol and common Oyl of Turpentine, as I bought them at the Druggists: these I put together very slowly, (for that circumstance should not be omitted) and obtain'd according to my desire an opacous and very deep-colour'd mixture, whose almost Balsam-like consistence was much thicker than either of the Liquors that compos'd it. (The like Experiment also successfully try'd with some other Chymical Oyls, but found none preferable for this purpose to Oyl of Turpentine.) And to make it probable that the disposition of these Liquors to mingle thus presently together depended much on their Texture, we made the mixture be warily distill'd over, (for else the Experiment will scarce succeed) and thereby obtain'd, (as we elsewhere mention to another purpose) a certain gross substance, which was that which
seem'd

seem'd to mediate the former union betwixt the two Liquors. For this substance being separated, and thereby the Texture of one of the Liquors (or perhaps both) being chang'd, the Liquors (which came over very clear into the Receiver) swome upon one another; nor have I since been able by shaking them together to confound them for any considerable time, but they presently part again, and do to this day remain distinct as well as transparent. But after having forgot to set down these things in their proper place, I must not forget also, that to employ here more words about them were to digress.

To this then annex we, that the Liquor we elsewhere mention our selves to have distill'd from Benzoin, has been and is still subject to much more frequent vicissitudes of Fluidity and Firmness; for part of it all the year long continues in the form of a blackish Oyl, and the rest according as the season of the year or of the day makes the weather cold or hot, frequently changes its Texture, sometimes appearing perfectly the same with the newly-mention'd Oyl, and sometimes shooting into clear and variously-shap'd Crystals, which fasten themselves to the bottom and sides of the Vessel, till a warmer part of the day or of the Season resolves them again into a Liquor. And these two last Observations may also serve to confirm what we formerly taught, that the Fluidity of some bodies depended almost wholly upon the various agitation of their parts: for in these instances the parts of the Anniseeds and those of the Benzoin, upon the operation or absence of the languid heat of the ambient air, sometimes agitating them, and sometimes suffering them to rest, did constitute a fluid or a consistent Body. And having thus taken notice of this upon the by, we will adde to the other Examples mention'd under this second head, that which it afforded us to our present purpose by Salt-Petre, which being dissolv'd in a sufficient quantity of common water, will seem to be lost

in it, and to constitute with it one uniform fluid substance; but if a competent quantity of that water be boil'd (or permitted to exhale) away, and the remaining liquor be suffered to rest a while, especially in a cool place, the saline particles will be re-uniting themselves and by the exclusion of the aqueous parts, constitute stable and determinately-figur'd Ice-icles or Crystals.

The consideration of this may suggest to us another way, that seems quite contrary to the former, whereby some bodies may become firm and solid, and that is by the intermingling of a due proportion of water or some other Liquor. For, though the small parts of such fluid Bodies, being themselves in motion, are apt to give those of others such an agitation as we have formerly taught that Fluidity principally depends on; it seems that the admission of any Liquor must rather conduce to the making of a body fluid than consistent; yet if we consult Experience, it will instruct us otherwise; for when I have taken either an equal or a double weight of Oyl of Vitriol and distill'd it warily from running Mercury; very much the greater part of the Liquor would come over, and leave behind it a very white Powder considerably fixt. And if we examine that familiar Production of Chymistry, *Mercurius dulcis* (which they now use to make by subliming of together two parts of crude Mercury, with but one of Sublimate, which consists chiefly of Mercury already) we may find that in That which is counted the best, the fluid Body of Quick-silver is so contex'd with the Salts it carries up in Sublimation, that the dry and brittle Body they compose may contain far more (perhaps twice more) Quick-silver than Salt. And other Experiments may persuade us, that the mixture of a convenient Liquor may cement bodies into one hard Concretion, which would scarce be compacted together othrrwise. Nor is it against reason that it should be so; for there may be differing quali-
fications

fications required to a body whilst it is constituting, and when it is constituted, and though the motion of the parts that make it up, oppose the firmness of a formed body, yet it may conduce to the making of a firm body: for when a great many hard Corpuscles lye together loose and incoherent, they do, as we formerly noted, emulate a fluid body; whereas by the mixture of a Liquor, those loose Corpuscles being for a while dissociated and put into motion, they may after many Evolutions apply themselves to one another after that manner that is most requisite to make them touch one another closely, and according to a greater surface. Whereupon it often follows, that the Liquor in which they did formerly swim is either squeez'd out upon their closing, or else so dispers'd in small particles, and dispos'd of among those of the harder Corpuscles, that they are unable to agitate them, or prejudice their mutual cohesion.

And here to dilucidate the subject under consideration by an instance that seems very pertinent to it, we will make a further use of the Experiment formerly mention'd touching the burning of Alabaster: For if the powder, after it has done boyling and has been sufficiently burnt, and kept some hours (the most experienced Artificers observing that it is not so convenient to employ it presently after it is taken off the fire) be well beaten and tempered up with fair water almost to the consistence of thin pap, if the powder have been rightly prepar'd and skilfully temper'd, you shall see that fluid substance in a few minutes of an hour begin to set (as the Trades-men speak) that is to exchange its Fluidity for Firmness, so that if it were before cast into a mould, it will perfectly retain the figure of the internal surface thereof.

Now that in our mixture there is for a while such an agitation of the hard parts produc'd upon the affusion of the water, and afterwards an exclusion of the superfluous water,

ter, we may confirm partly by this, That when any considerable quantity of burnt Alabaster is temper'd up with water, the mixture after a little time grows sensibly hot, and sometimes continues so for a pretty while: and partly also by this, That having purposely for tryals sake fill'd a new and good Glass-Vial, containing about half a pint, or pound, with the mixture we speak of, and when it was top full, stop'd it up very close, the liquid mixture within less than half an hour crack'd the Vial (though standing in a window) in several places, and at those crevices discharg'd it self of about a spoonful of clear water, the remaining mixture retaining perfectly the figure and dimensions of the Vial, and growing as hard as Chalk or somewhat harder, insomuch that we were fain to imploy several strokes with a strong Iron to divide the mass.

And let me here adde, that some other substances may this way afford much solider Bodies than burnt Alabaster does: and therefore it may be a thing of good use to enquire out and try what other Bodies, easily to be procur'd, may be thus brought to a new and lasting Solidity. For the Learned Hydrographer; *Fournier*, speaking of those Damms or *Dignes* (as he calls them in his Language) which are sometimes made in the Sea to secure Shipping, (as I have seen at the Port of *Genoa* and elsewhere) after having told us that the Romans made the fairest Harbours in the World by the help of a certain Sand to be met with at *Cuma* and *Puteoli* in the Kingdome of *Naples*, which Sand

Hydrograph. Du
P. G. Fournier,
Lib. 2. Cap. 6.

mingl'd with a third part of Quick-lime acquires in the water a flint-like hardness; subjoyns this Observation of his own, *Jayven*, &c. that is, I have seen (sayes he) in *Flanders* near *Tournay* a certain sort of ashes of Lime made of Marble, which was excellent for any kind of work made in the water. For having made a Bed of great stones, they cast upon them whole Baggs full of such ashes instead of Mortar,

tar, and the water betwixt the stones having temper'd up these ashes, petrify'd them to that degree, that in a short time they became as hard as Marble. Thus far He. But to pursue our former Discourse.

That also which we intimated of the conduciveness of the various tumblings to and fro of the hard particles to their uniting into one firm concretion, seems confirmable by what we have observ'd in some saline Liquors, especially certain parcels of Spirit of Harts-horn, which whatever were the constitution of the ambient air, remain'd fluid some of them for many months, after which the saline Corpuscles began to shoot at the bottom of the remaining Liquor into exquisitely-figur'd Crystals, which at length grew copious enough. For this spontaneous coagulation of the little saline Bodies happening so late, it seem'd that it was preceded by almost innumerable evolutions, which were so many and so various, that at length the little bodies came to obvert to each other those parts of themselves by which they might be best fasten'd together and constitute a firm body. Which conjecture seem'd the less improbable, because we could not well imagine that this coagulation proceeded (as that of dissolv'd Allum and other Salts is wont to do) from the evaporation of the superfluous Liquor; for the Glasses wherein what we have mentioned happen'd being carefully stop'd, there was no danger of such an avolation, and if any thing could get away, it must have been the subtil peircing and fugitive Spirit, (which indeed, as my Nose had inform'd me, does oftentimes penetrate ordinary stopples) for the flying away of those volatile parts would only have left the remaining Liquor more aqueous. And 'tis well known to those that deal with such kind of Liquors, that the more aqueous they are, the less apt they are to Crystallize. And however it will serve our turn, that there was but an insensible diminution of the Liquor upon the

recesses of whatever it was that got through the Cork.

To the same purpose I remember also, that having in a Crystal Vial carefully kept a pretty quantity of well-colour'd Tincture of Amber, made with pure Spirit of Wine, it remain'd fluid for a year or two, and during that time presented us with a strange Phænomenon that belongs to

* This Phænomenon is partly describ'd at large in one of the Authors Physico Mechanical Experiments.

other papers. * But having been absent for two or three years from the place where we lock'd it up, we found, when we came again to look upon it, that that though it had formerly remain'd fluid so long, yet several yellow lumps of Amber, almost like Beads, with one side flat, had here and there fasten'd themselves partly to the bottom, and partly to the sides of the Glass: the rest of whose internal surface continues yet transparent.

Another thing whereby bodies become stable is, the admission of adventitious Corpuscles into their Pores and recesses. And of the wayes by which these foreign Corpuscles may bring the substance they invade to be compact, these four appear the chief.

First then, the adventitious Corpuscles we speak of may produce stability in the matter they pervade, by expelling thence those voluble particles which, whilst they continu'd in it, did by their shape unfit for cohesion, or by their motion oppose the coalition, or disturb the Rest of the other particles whereof the Body consisted. But of this having already discoursed, proceed we to what is to follow.

In the next place then, foreign Bodies may contribute to the stability of a substance they get into, by hindering the motion of the little Bodies that constitute it.

And thirdly, such advenient Bodies, especially if they be not of the smallest size, may produce a firmness in the substance

stance which they get into, by constituting with the particles it consists of, Corpuscles more unapt for motion, and fitted for mutual cohesion.

These two we mention together, because that very often Nature employs them together for the introducing of stability into Matter.

To these seems to be reducible the way of turning the fluid body of milk into cruds by the mixture of a little Runnet, whose saline particles pervading the body of the milk, do not only make a commotion in the parts of it, but fasten the branched particles of it to one another, and with them constitute a body of another texture than was the milk; and the weight of these crudled bodies reducing them by degrees into a closer order, does, whilst it presses them together, squeeze out the thinner and more serous Liquor, which the Runnet was unable to coagulate, and which being thus sever'd from the grosser parts of the milk, may well be more fluid than milk it self is wont to be. And that there is some coalition of the particles of the Runnet with the coagulated ones of the milk, may appear by the complaints that Houswives sometimes make of their Dairy-maids, that the Cheeses tast too strong of the Runnet, when too great a proportion of it has been mingled with the milk. And though we ascrib'd the crudling of the milk to the saline particles of the Runnet, we ignore not that not only common Runnet, but also diverse juices of herbs will crudle milk, as is well known in those parts of *Italy* where Cheese is made without Runnet. But we made especial mention of the saline Corpuscles of the Runnet, because really Houswives are wont to salt it, and because saline Liquors do manifestly and powerfully operate in the coagulation of milk, which may be crudled by juice of Limons, and I know not how many other Acid Salts. And to manifest yet further the coagulative power of them, we have sometimes in about a minute of

an hour arrested the Fluidity of new milk, and turn'd it into a crudle substance, only by dexterously mingling with it a few drops of good Oyl of Vitriol. But of the effects of various Salts upon milk we elsewhere may, and therefore shall not now, discourse.

Between this last recited Experiment, and the two following ones, 'twill not be improper to insert the immediately ensuing one, for the Affinity which it seems (in different respects) to have with both.

I remember (then) that I divers years ago prepar'd a Salt, which either was, or at least answer'd well to the qualities ascrib'd to that which is now called *Glauberus's Sal Mirabilis*, which seem'd to have in it a coagulative power, in reference to common Water. For whereas Salt of Tartar, Common Salt, Nitre, &c. being dissolv'd in Water, do upon evaporation of a sufficient quantity of that Water, recover indeed their pristine Saline Forms, yet they do but coagulate themselves, without concoagulating with them, either any Water, or at least so much, as Chymists have thought worth the taking notice of: Whereas this Salt we speak of, being prepar'd for the purpose, and dissolv'd in a convenient quantity of Water, does upon its recoagulation so dispose of the aqueous Particles, among its own Saline ones, that if the Experiment be well and carefully made, almost the whole mixture will shoot together into fine Chrystals that seem to be of an uniform Substance, and are consistent enough to be even brittle, and to endure to be pulveriz'd, sifted, &c. though the Concretion may have such a Proportion of Water in it, that (as I remember) when the Experiment succeeded well, from three parts of Water and but one of Salt, I had about four parts of Chrystals.

I need not tell you that this Salt seems to have a somewhat more then ordinary Resemblance of a true Coagulum, since it reduces so much water into a stable consistence,

yet:

yet it does in no contemptible proportion materially concur to the Body produc'd. But I may hereafter (which I must not do now) entertain you about a Salt of a differing kind from this; and which put me upon considering, whether there may not be a Coagulum more properly so call'd of Common Water, which may in a very small proportion operate upon a great quantity of that Liquor, as Runnet does on Milk.

I have not yet examined whether it will be sufficient to refer meerly to the second and third ways lately mention'd of making Bodies become stable in the Phænomena I am about to speak of, or whether it may be reasonably suppos'd (and added as a fifth way) that the Bodies to be coagulated may (in great part) be brought to be so; by so acting upon the Bodies to which they are put, that the Agent Liquor (if I may so speak) does by its action communicate to the subject it works on, or lose upon some other account some subtile parts whose absence fits the dispos'd remaining Fluid for such a Cohesion, as may suffice to make a Body be (though very soft, yet) consistent. But however it will not be amiss to take some notice of Effects, which, what e're the cause be, belong to the History of Fluidity and Firmness.

I some years since prepar'd a Substance of a whitish colour, which would not only destroy the Fluidity of some other Liquors, but would give a consistency to a notable proportion of Oyl of Vitriol it self, though the parts of this Liquor be presum'd, upon the score of its corrosiveness, and its aptness to grow very hot with many other Bodies and make them smoke, to be very vehemently agitated.

And I remember that I sometimes shew'd the curious a Glass Vial well stop't, upon the bottom of which lay a little of this newly mention'd whitish powder, over which there was a considerable proportion of Oyl of Vitriol in a

consistent Form without seeming to have any thing to do with the Powder, as indeed it had been only pour'd upon it, and suffer'd to stand in the cold for some time (which if I mistake not was a day or two) at the end of which the above mention'd change was wrought on the Liquor by the powder which did not appear to be dissolv'd thereby. Which Phænomenon seem'd indeed to argue, that there happen'd in this Experiment (that was not the only one of the kind I then made) something like the coagulation formerly mention'd of Quicksilver by the Vapour of Lead, some subtle parts of the Coagulator, if I may so call it, invisibly pervading the Liquor whose Fluidity was to be suspended, though it seem not improbable to me, that the effect produc'd might depend upon both causes, this newly express'd, and the other a little abovemention'd; where I guess'd that a change of Texture, and thereby of Consistence in the *Menstruum*, might be the result of the Operation of the *Menstruum*, and the Body it acts upon. And because this powder is not so easie to be prepar'd, I shall adde that you may (though not so well as by the newly mention'd way) see the Coagulation of a *Menstruum* upon a firm Body which it does not seem to dissolve by the ensuing Experiment, Take Crystals of Salt-Petre very well dryed, but not powder'd, and gently pouring on it in a Glass Vial some good Oyl of Vitriol till it swim about half an Inch, or perhaps more above the Salt, leave the Vial clos'd with a cover of Paper in a cool quiet place, where it may not be shaken; and if the Tryal succeed with you as did it with me, the Liquor will, though slowly, so settle it self about the Nitre, that though you incline the Vial to any side (or perhaps turn it upside down) it will not run out; and I have sometimes taken notice of little saline Bodies, and as it were Fibres, that seem'd to keep the parts of the mixture united together. I made also some other Tryals to coagulate unflegmatick *A. F.* upon Nitre
and

and some other Bodies, the Phænomena of which Tryals, did not oblige me to renounce the lately mention'd Conjectures about the causes of such changes of consistency in Liquors, as I have been speaking of: For I still think it highly probable that the best Coagulator I have met with acts but as a finer sort of Runnet, which in an inconsiderable quantity really disperses material parts of it self through the Liquor to be wrought on, though these when the Coagulator is a consistent Body, be perchance so few or subtle as not to make any Visible diminution of the Body it parts with.

A more eminent Example to our present purpose may be afforded us Sometimes (for I am sure the Experiment will not Always succeed) by the notable way of coagulating Quick-silver, and thereby turning it from a fluid into a firm body by the vapour of melted Lead, in which when it is taken off the fire (but before it be quite grown hard again) a little cavity must be made with a pebble or a stick, that the Quicksilver tied up in a rag may be nimbly put into that hole, and be congeal'd by the permeating steam of the cooling lead. Which Effect may be less hopefully expected by the way wont to be prescrib'd by Authors (most of whom I doubt never made tryal of it) then by another that I have practis'd and may on another occasion shew you. And that some metalline steam does really invade the Quicksilver, seems probable by the wasting of Lead by fusion, and by the operations ascrib'd by Chymists to the fume of Lead upon Gold, about which I may elsewhere tell you what is come to my Knowledge.

And I remember that not long since, an ingenious Physician of my acquaintance keeping some Lead long in fusion to reduce it *per se* into a Calx, and holding his head often over the melting pot to observe the alterations of the metal, was suddenly purg'd diverse times both upwards and downwards, which both he and I ascrib'd to the Sa-

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turnine exhalations. And though I suspected the Congelation formerly mention'd might proceed from the egress of some subtil substance that formerly agitated, but after deserted, the Mercurial Corpuscles; yet that the Concretion of the Quick-silver might be effected by some benumbing vapour of the Lead, seems confirmable by a notable accident that befel that famous Geometrician Doctor *Wallis*, who related it to me as a Phænomenon he knew not well what to make of; namely, That he and other Learned Men at *Oxford* being minded to make the Experiment under consideration, they found that upon the first fusion of the Lead the immersed Quick-silver was very well coagulated by it; but when they came to melt it the second time, and put new Quicksilver into it, the Experiment would not succeed, at which they wonder'd, finding by Tryals that the Lead might be so easily deprived of its power of hardning Quick-silver. That this Observation will always hold true, I am not apt to believe; but that such Learned and Candid Naturalists should either be mistaken in making it, or mis-relate it, 'twere injurious to suspect: wherefore supposing that to have then at least happen'd which one of them registred in writing, and more then one of them told me; it seems to countenance what we have deliver'd, and looks as if according to our Doctrine there were in Lead a coagulative steam or Spirit, and yet (at least in that parcel of metal) in so small a proportion, as that it almost totally dislodges or spends it self upon the first opportunity it meets with of passing into Quick-silver.

We have elsewhere to another purpose mention'd our having sometimes (for, as we there advertise, it will not always succeed) made an Experiment which seems of kin to the former, and may give much light to the matter under consideration, and it was this; We pour'd upon *Aqua fortis* common Sallet Oyl, which floted together at the top
of

of it, but after some hours had its texture so changed by the ascending steams or other subtle insinuating particles of the saline Liquor, that it was turn'd into a white consistent (and sometimes a brittle) body like Butter, remaining all in one Cake on the top of the *Menstruum*. And the like Experiment (but in a longer time) we have perform'd with express' Oyl of sweet Almonds instead of common Oyl.

And to shew further, how much the operation of the same visible Agent may be diversified as to the Production of Fluidity or Firmness, according to the differing dispositions of the Bodies it acts on. I have sometimes (I remember) taken the *same Aqua Fortis*, or Spirit of Nitre wherewith I had coagulated express'd Oyl of Olives, and having pour'd it off from the Butter-like Substance: I cast into it some good Camphire, which without heat was thereby reduc'd into an Oyl, that retain'd a distinct Superficies from the *Menstruum* which it swam upon, and would not incorporate with, so that the same numerical *Menstruum* without the help of any degree of fire, turn'd a brittle Body into a Liquor, and the Liquor into a brittle Body (for such is the Substance that may be made of common Oyl, if it be suffer'd to float long enough upon the coagulating Liquor) which brittle Substance (to adde that upon the by) seem'd to have receiv'd a more durable alteration from the steams of the *Menstruum* then was expected. For not only when melted with fire, it would upon refrigeration recover its consistence without becoming again fluid, as when 'twas in the form of Oyl; but I made a Tryal or two without success to reduce it to a Liquor by mixing it with Oyl of Tartar *per Deliquium*, which, you know, has a great Faculty to find out and mortifie Acid Spirits, such as those of the Nitre or *Aqua Fortis* that had (whether as meer Acids I now examine not) coagulated our Oyl.

The fourth and last way whereby Corpuscles entering from without into a Body may give it a stable consistence, is by making such a commotion in the parts of it, as may make them apply themselves one to another according to a greater surface, or otherwise complicate and dispose them after the manner requisite to make them stick together.

This way of making Bodies become consistent, is seldom or never employ'd by Nature without the concurrence of some of the other ways already mention'd: but we have distinguish'd it from the two last recited, because in them we suppose that some of the adventitious Corpuscles are stop'd in the body to whose firmness they conduce, and (though perhaps but in a very considerable proportion) do concur to make it up; whereas here we suppose that without materially concurring to constitute the body they work upon, they do only agitate and variously move the particles it consists of, perhaps breaking some, bending and twisting others, and in a word so altering the Texture, that the parts that did formerly either move separately, or adhere together but loosely, are now reduced to a closer order, or a more implicated Texture, and thereby more firmly connected to one another. That the bare disposition of the parts of a body in reference to each other, without any addition of foreign matter, may do much towards stability, we may see both in some examples formerly mention'd, and in Osier wands, which when lying loosely in an heap together may each of them very easily be dissociated from the rest; but when they are breaded into a Basket, they cohere so strongly, that when you take up any one of them, you shall take up all the rest. To which may be added those many obvious though perhaps unheeded Instances wherein by the bare Texture of the slender hair or filaments whereof Wool or Silk consists, Cloth, Silk-stockings, and many other durable

Garments are made by illiterate Tradesmen.

We may also observe the force of bare motion in altering the texture, and thereby the consistence of bodies by the common way of Churning, for there the external impulse of the Churn makes a great commotion in the parts of the Cream, and tumbles and shuffles them perpetually to and fro among themselves, whereupon it happens, that the more branched Corpuscles meeting with one another are intangled, and thereby separated from the rest, and after many occurrences all these parts are at length fasten'd to one another, and excluding those of the Butter-milk, which seem not so conveniently shap'd for mutual cohesion, do constitute Butter; which is wont to be made yet more consistent, or rather more compact, by being beaten or otherwise compress'd, as the parts thereby reduced into a closer order squeeze out the fluid Butter-milk that was intercepted among them.

It will perhaps be thought more strange that a fluid Body, nay a distill'd Liquor which is very volatile and passes for simple and Homogeneous, and is at least far less compounded than milk, should by motion, without the mixture of any new matter, be made coherent: and therefore I hope that it will not only second the Example newly alleg'd, but likewise confirm some main points of our Doctrine touching Firmness, if we observe that ev'n the Chymical Oyl of Turpentine, which passes for one of the Principles or Elements of that Body, may be in great part, if not wholly, coagulated without addition. And yet (not to anticipate what I may have occasion to deliver elsewhere concerning this Experiment) I shall now only relate, That enquiring a while since of a very expert Chymist, whether he had not sometimes observ'd (which I have often done, as I elsewhere declare) Oyl of Turpentine to begin to coagulate if it were often distill'd; he went with me to his Laboratory, and there let me see in a Receiver some Oyl of Turpentine

which he had often distill'd over *per se*, in good part coagulated into a whitish and consistent Body: affirming also to me, that he had sometimes by frequent Distillations, without Addition, obtain'd from clear Oyl of Turpentine a far greater proportion of such a stable substance. Whose consistence, whether it should be ascrib'd to the fires breaking the Oily Corpuscles into parts more fit for mutual cohesion, or whether it proceed from a new texture of the same Corpuscles, only chancing by those various Evolutions to be dispos'd after such another manner as to complicate or otherwise connect them, I need not now spend time to enquire; since 'tis enough for my present purpose, that in this example we have one that declares, how much ev'n motion without the Addition of any sensible substance may in some cases conduce to Firmness.

And here to illustrate our Doctrine about this Quality and Fluidity, by shewing what the intestine motion of the parts, even without the assistance of adventitious heat, may do, to make a Body change its consistence according to the previous disposition of the matter, and become of firm, fluid: as we lately saw Oyl of Turpentine made of fluid, firm. I will adde on this occasion what I observed of Oyl of Wax distilled in a Retort with an Additament of the like nature with that I formerly mentioned, when I spoke of the fluid Oyl drawn from Oyl of Olives. For this Oyl of Wax, though at first it came over for the most part, if not totally, in the form of a Butter; yet by standing on a shelf (and that not in a hot place, as a Stove or Laboratory.) I observed it little by little to resolve into a transparent Oyl, and purposely enquiring of him that lookt after it, whether or no this effect might not be ascribed to the increased warmth of the Weather, he assured me of the contrary, having taken notice what effects the changes of Weather had upon it.

But what if we should say, that fluidness and stability depends

depends so much upon the texture of the parts, that by the change of that texture the same parts may be made to constitute either a fluid or a dry body, and that permanently too? These last words I adde, because of what may be said to this purpose concerning the change of water into Ice, and Ice into water, and of metals into fluid or hard bodies, by fusion and refrigeration: for in these examples the acquired hardness of water and fluidity of metals may be presently lost upon the bare removal of those bodies into a temperate air; whereas in the instance we are to give, the acquired texture is so durable, that without an extream external violence, such as would destroy most other stable bodies, it is not to be destroyed. And this instance is afforded us by that admirable Repository of Natures wonders, Quick-silver: for if some Ounces of this fluid mineral be put into a convenient glass vessel, and that vessel be first exactly stop'd and kept for 6, 8, or 10 weeks (or longer, if need be) in a sand Furnace whose heat may be strong and constant, the Corpuscles that constitute the Quick-silver will, after innumerable revolutions, and perhaps bendings, twistings, and other changes, be so connected to one another, that instead of a fluid Body, they will appear in the form of a red powder, that Chymists Precipitate *per se*: which change is so unexampl'd, that though among the more curious Spagyristis it be very well known, yet many Naturalists cannot easily be brought to believe it; whom to convince of the possibility of it by a much less tedious preparation, I take half a pound or a pound of Quick-silver, and with a strong fire distil it out of a glass Retort, and for the most part there will remain in the bottom and about the sides of the vessel a little red powder, which seems to be nothing but part of the fluid body (most exposed to the action of the fire) turned into a dry one in eight or ten hours space.

After what manner the fire produces so odde a change:

in the Quick-silver, I do not presume to know. 'Tis true, that though the parts of Liquors do, as we have formerly taught, touch one another but in part of their superficies, yet they all of them seem to have some degree of viscosity, or some slight and loose complication or other kind of Adhesion of parts, as appears by their being so easily contexted into those thin membranes or films we call bubbles, in so much that not only Spirit of Wine, that seems the most light, and most fluid of Liquors, will afford bubbles, but (what may seem strange) we have divers times purposely observ'd, that Quick-silver it self, as ponderous as it is, especially being suffer'd to fall in a slender stream into a vessel almost full of the same mineral, will afford bubbles numerous and large enough, although (as those also of the Spirit of Wine) quickly vanishing.

And hence it might be imagin'd, that in the operation we are treating of some such change is made in the Quick-silver, as we formerly observ'd to be made in the white of a Egg, when by a new disposition of its parts, either heat or beating it makes it a kind of stable body, or else it might be pretended, that there is a variety of parts argu'd to be in Quick-silver by the great variety of its effects upon other bodies, and that by the frequent evolutions which the fire makes of those parts among themselves, they come at length to be so appli'd to one another, that either they lock into each other as it were, or slip upon one another's surface in such a manner as that as much of their surfaces immediately touch one another as is requisite to make them cohere, as we formerly mention'd of several very smooth pieces of glass mutually adhering without any other Cement than the congruity and immediate contact of their surfaces. But though these Conjectures and divers others might be propos'd, yet I fear all of them would prove but meer Conjectures. Nor were we much assist'd to make better by looking upon our Mercurial precipitate in one of
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the best Magnifying Glasses in the World; for what we there discover'd was only, that the red powder had in it many Corpuscles of sundry other colours, and that the little grains of powder seem'd to be of no determinate shape, but look'd like slender fragments of Red Coral: and having but some small dust of a shining precipitate of Gold and Mercury into the same Augmenting Glass, all we could discern was, that the little grains of this precipitate differ'd from those of that made of Mercury alone, in that these (which a Chymist would take notice of) were so transparent throughout, that one would verily think he beheld the best sort of those precious Stones Gold-smiths call Granats. But though we pretend not to make out how the new Texture is produc'd in the Quicksilver, yet to make it still more evident that its change of consistence proceeds from its change of Texture, we will adde, that having a great curiosity to try whether our powder could not be made fluid again, I procur'd some precipitate *per se* of a Person who formerly lived with me, and was expert in many Mercurial operations, and presented me some of his own making: this being weigh'd and put into a convenient glass was carefully press'd with a naked fire (which should be stronger than that wherewith it was precipitated) and at length it rose by degrees in fumes, which settl'd in the neck of the Glass in many drops of reviv'd running Mercury; all which being collected into one, we found that there wanted but about a sixth or seventh part of what we had put in, and we suppos'd we should not have wanted that neither, but that the vehemence of the fire had melted the glass, which swallow'd up a part of the powder that made a great shew through it, after what was colliquated had been remov'd from the fire.

This Experiment brings into my mind another that was judg'd uncommon enough, and it was This: Being not long since master of about half an ounce of a certain Mer-

cury;

cury, which some ways of examining it that I had employ'd, induc'd me to think Mercury of Saturn; I imagin'd (for some reasons) that it might be made very serviceable to confirm our Doctrine touching Fluidity and Firmness. And accordingly I found upon Tryal, that I could, barely by shaking it long, reduce it to a black powder: in which form it would continue as long as I please to let it do so. And when to the By-standers there appear'd nothing in it that gave suspicion of a fluid Body, I could in a Trice, only by dexterously rubbing it in a small Marble Mortar, reduce it little by little into running Mercury, as it had been before. Which quick passage from one quality to another, being made, not only without the help of Fire, but without adding or taking away any visible Substance, prov'd no ignoble Instance, how much Motion and Rest, and the thence easily resulting Texture of the Component Corpuscles of a Portion of Matter, may contribute to its Fluidity or Firmness.

From the Experiment of precipitating Quick-silver *per se*, and from some other things, partly deliver'd already, and partly to be deliver'd by and by, we may learn what to think of the opinion of some Eminent Modern Philosophers who teach, that a fluid body is always divisible into bodies equally fluid, as Quantity into quantities; as if the particles of fluid Bodies must also be fluid themselves: for by them it seems to appear, that Quick-silver, and some other actually fluid Bodies consist very much of hard Corpuscles, since by the change of their Texture they may be deprived of their Fluidity and become stable. We see also that the stiff and solid particles of Salts dissolv'd in common water, and of Silver dissolv'd in *Aqua fortis*, being by those Liquors sufficiently dissociated and separately agitated, do with them constitute fluid Bodies. And we have elsewhere mention'd to another purpose an Experiment which may not imper-

tinently

tinently be repeated here, namely, that by putting together into a glass Retort one part of Quicksilver and four of common Oyl of Vitriol, and distilling them in a sand Furnace with a strong fire, there remain'd in the bottom of the Vessel a ponderous Calx or Powder, so far from being fluid, that it was but in part dissoluble in water: and that which seems to prove that in the very liquid Oyl of Vitriol, though a distill'd Liquor, the saline Corpuscles that chiefly compose it, do retain their stiffness (generally to be found in undistill'd Salts) is, that by steeping our Calx in fair water, we could separate from it a considerable quantity of particles, which upon the evaporation of the water coagulated into store of saline and brittle bodies. And that these proceeded rather from the *Menstruum* than the metal, we were induc'd to think, by observing the dry Calx, before any water was pour'd on it: for though the saline part of the Mixture did not weigh (perhaps any thing near) so much as the Mercurial distinctly did, yet the Aggregate or Mixture did weigh a great deal more than the Quicksilver did when it was put in; and the Oyl of Vitriol that was abstracted, a great deal less than it did before it was committed to distillation. Nay, I once or twice observ'd in a glass, where I kept a quantity of Oyl of Vitriol, that there did spontaneously fasten themselves to the sides little saline Crystals, which when I took out, I found hard and brittle; but when I had for tryal sake expos'd them to the air, they presently resum'd a fluid form, and appear'd to be Oyl of Vitriol. In the Observation also lately mention'd concerning the spontaneous coagulation of Spirit of Harts-horn, it seems evident, that Bodies which are all or most of them hard, and appear so when they are commodiously connected to each other, may yet constitute a fluid body when they are reduc'd to sufficient smallness, and put into a convenient motion. And indeed, if the least par-

ticles of fluid bodies were not (many of them at least) indowed with their determinate bigness and shapes, but that such fluid bodies could be always divided into particles fluid also, how comes it to pass that some Liquors cannot pierce into or moisten some bodies which are easily pervious to other Liquors? for if the particles of the excluded Liquor were of necessity always divisible into fluid ones, there seems no reason why they should not be sub-divided into so very small ones, as that no pores can be suppos'd little or odly figur'd enough to keep them out.

'Tis true indeed, that as it is hard to demonstrate, so it is not easie to disprove, that the matter whereof fluid bodies consist is capable of being indefinitely divided: and it may be granted too, that by how much the smaller parts a body is divided into, by so much the more easily, *Cæteris paribus*, are the parts of that body to be put into motion. But this divisibility of a fluid body into perpetually lesser and lesser parts belongs not to it properly as it is Fluid, but as it is a Body; such divisibility, if suppos'd true, being a primary affection of matter it self, and belonging as well to those portions of it that are hard as to those that are fluid. And though it were admitted, that such an endless division as is presum'd might be made Mentally (as they speak in the Schools) that is by the thought or operation of the mind, yet it would remain a great question whether or no Nature does actually so far mince and sub-divide Bodies: as may appear by what has been freshly noted. And however, it is not only requisite to the constitution of a fluid body that the parts of it be small enough, but that they be also actually mov'd. For we observ'd not long since, that the dust of Alabaster put into motion did (though its Corpuscles were not insensible) emulate a fluid Body, and immediately ceas'd to be fluid when they ceas'd to be agitated: whereas the particles of water, as minute and apt as they are to constitute a fluid substance, do yet
make

make that hard and brittle body we call Ice, when those little particles upon what account soever are reduced to be at rest.

By what has been hitherto discours'd, we may also be assisted to judge of the Doctrine of the Chymists, who teach that in all Bodies, Coagulation, Stability, Hardness and Brittleness depend upon Salt: for though what above has been said of Crudling of milk by saline Liquors, and the hardness and brittleness obvious in Salts themselves, may keep us from denying that the saline principle is very powerful in the coagulation of some bodies, and does produce much firmness or even brittleness in many or most of the concretes wherein it is predominant; yet this hardening power of Salt seems not to proceed from any peculiar and inexplicable property it has to coagulate other bodies or make them compact, but from the shape and motion of its Corpuscles, which it seems are more fitted by Nature than those of many other Concretes to insinuate themselves into the pores of other bodies, and fasten their particles to themselves, and to one another, either by wedging their Corpuscles together, or by their stiff and slender parts, or their sharp angles or edges piercing diverse of them together; as when many Pieces of Paper are kept from scattering by a Wire that runs through them, or as when a Knife takes up at once diverse pieces of Bread and Meat by being stuck into them all. But whensoever there is in the constituent parts of the body a sufficient fitness and disposition to adhere firmly to one another, Nature may of those parts compose a stable body, whether they abound in Salt or no, it not being so much upon Chymical Principles, or ev'n upon the Predominancy or Plenty of any one Ingredient, as upon the shape and motion of the component parts of bodies, that their Fluidity and Firmness depend. I will not here urge that Salts are generally reducible by an easie mixture with water into the form of Liquors; nor

that Sea-salt, Salt of Tartar, and diverse other sorts of Salts, will of themselves, ev'n in the Air, if not very dry, assume the form of fluid Bodies; nor yet will I press the shortly to be mention'd Example of Coral, which is confidently affirmed to be soft whilst it remains in the Salt water, and to grow hard when taken out of it. I will not here, I say, press these and the like Arguments, but content my self to have hinted them, because they are such as I cannot well in few words make out and vindicate. Wherefore I shall rather demand, what Salt can be made appear to pass out of the body of melted Lead into that of Quicksilver, to perform in it the coagulation abovemention'd? What accession of Salt is there to be observ'd, when running Mercury is precipitated *per se* into a powder? and how will it be prov'd, that when in a well-stop'd glass the whole body of water is in frosty nights turned into firm Ice by the cold of the ambient air, that coagulation is perform'd by Salt, it having not yet been made appear by Chymists, that either Salts or even the distill'd Spirits of them can penetrate, without a kind of Prodigy, the narrow pores of unheated glass? It is usually observ'd in Eggs, that though at their first coming out of the Hens belly, the shells are soft, yet soon after they grow hard and brittle; and yet it appears not how the saline Ingredient is encreas'd to effect this speedy induration: and (to subjoyn that by the by) albeit I am not averse from thinking that the coldness of the outward Air, and its imbibing some of the loofest of the moist parts of the soft Egge-shell, may concur to this effect; yet there are many Observations of Egge-shells that have been found hard in the womb of the Hen. And I well remember I have taken notice, that diverse Eggs not yet laid, but found at one time in the body of the same Hen, were each of them furnish'd with a compleat and brittle Shell. But I think I can draw a much stronger Argument against the Chymical opinion

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from the consideration of an Egg : for I demand what plenty of Salt can be made appear to pierce the hard shell, and more close-wrought membrane that both lines it and involves the Egg, especially since 'tis certain, that in *Egypt* and diverse other places Eggs may be hatch'd by a temperate external heat without the Hen. And yet we may here observe, that the same internal substance of the Egg which at first was fluid, the yolk and white that compos'd it being so, is upon the exclusion of the Chick turn'd almost all of it into consistent Bodies, some of them tough, as the membranes and gristles of the Bird, and some of them harder and almost brittle, as his bones and beak ; and all this as we said without accession of new Salt. It would be hard for Chymists to prove, that Diamonds and Rubies, which are counted the hardest Bodies we know, (and at particular tryals of whose hardness I have sometimes wonder'd) do abound in Salt ; at least it will not be unreasonable for us to think so, till Chymists have taught us intelligible and practicable wayes of separating (at least some) true Salt from either of those Jewels. And it may be also doubted whether the blood of Animals when it is freest from Serum, do not (though a Liquor) as much abound with Salt as their skins or their flesh.

And since 'tis with Chymists that I am now Reasoning, I presume I may be allow'd to press them with Arguments drawn from some of the Eminentest Writers of their Sect. For the generality of Chymists, and even those that are by the rest, and themselves too, call'd Philosophers, not only granting, but asserting and maintaining the Transmutation of great quantities of Quick-silver and the other ignobler metals into Silver or Gold by means of the white or red Elixir, I shall demand of them whence it happens, that one grain of the powder of Projection can turn a whole pound of Mercury into true Gold or Silver, and consequently change a very fluid Body into a very firm one, though the

proportion of Salt employ'd to coagulate the whole Mass of Quick-silver would not amount to the six thousandth or seven thousandth part of the Liquor; though we should grant that the powder employ'd to work this marvellous change were all of it Salt, to which yet Chymical Writers seem to ascribe much more of the sulphurous Nature. And to this I shall adde, what the famous and acute *Helmont* does to another purpose relate upon the Experience of *Raymund Lully* and his own, concerning his prodigious Liquor, Alkahest; namely, that being abstracted from common Quick-silver, it does in a quarter of an hour coagulate it: and yet in this coagulation he points at this as a singular Phænomenon, that this Liquor which is as well immortal as exceeding saline, leaves nothing of it self with the Mercury on which it works, and yet so coagulates it, that he prescribes the making it into a subtil powder.

Helmont
de Febr.
cap. 14.

I remember also to our present purpose, that a Physitian of much veracity in what he relates, discoursing with me the other day about an odde preparation that he saw at the present Duke of *Holstein's*, (that Learned Prince and great Chymist) assur'd me that among other things he there took notice of a glass of Spirit of Urine, which in warm weather remain'd in the form of a Liquor, but in cold weather did totally coagulate into Crystalline salt: and being ask'd by me if he knew how this Urinous body had been prepar'd? he answer'd me, that the Duke caus'd Spirit of Urine exceeding rich in volatile Salt to be distill'd very many times; after every Distillation re-conjoyning all that came over in a Liquid with that which remain'd in a saline form, till by very frequent cohobations all the parts of the Urinous substance were brought to the union or coalition above-mention'd. What we may propose concerning the various consistence of the saline part of Urine upon our own knowledge, we shall for certain Reasons reserve for another place:

And

And on this occasion we will annex a few particulars, which may tend not only to the making of the Chymical Hypothesis about the coagulation of bodies doubtful, but to the confirmation of much of the Doctrine by us propos'd. The first shall be an Observation afforded us by the Art of making Sugar, wherein very great care is taken, that nothing acid (and especially juice of Limons) fall into the Caldrons or other Vessels wherein the juice of the Sugar-Cane is to coagulate into Sugar: for though acidity be generally by the Chymists ascrib'd to Salt, yet here the saline bodies are so far from promoting the coagulation of the saccharine srrup, that they would quite hinder it. And because that through the want of Sugar-Canes in these parts, we are reduced to take this Observation upon the credit of others, and because also in it self it seems somewhat strange, we will vouch for it two eminent Authors in whose Writings we met with it. The one is the ingenious French Publisher of the natural and moral History of those *American* Islands, commonly call'd by the French *Les Isles Antilles*, and by us the *Caribe* Islands, who describing particularly how his Country-men make Sugar in those parts, gives this caution towards the latter end; *Sur tout*, &c. that is, Above all, great heed must be taken to let no juice of Citrons (or Limons) fall into the Caldrons, for that would absolutely hinder the formation of the Sugar. The other is the diligent *Gulielmus Piso*, who having given us a particular account both by words and pictures of the way of making Sugar, tells us that, *Si momentum succi Limonis vel acidi quid injiciatur, sacchari consistentiam nunquam acquirat, sed in totum perditur.* To which I shall adde, that having purposely inquir'd concerning this Observation, it has been confirmed unto me by Persons that pretend more than ordinary knowledge of the Art of ordering Sugar: which likewise affords us another Observation

Hist. Moral.
Cap. 5.

Histo. Nat.
& *Med.*
Brasil. l. 4.
c. 1.

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not impertinent to the Theme we treat of; for the best Authors that write hereof inform us, that the juice squeez'd out of the Sugar-Canes is wont first to be boild and depurated in vast Vessels of Copper or Brass, whence it afterwards is convey'd to be further purifi'd and coagulated into smaller ones; and that whilst it is in the former, they use to pour upon it some very strong Lee to facilitate the separation of its feculencies, as in the smaller ones 'tis usual to pour a little Oyl or Butter upon the boyling juice, to keep the sirrurp from boyling over. Now that which they further observe to our purpose, is related almost after the same manner both by our French Author and by *Piso*, and by the latter of them in these words, *Observatu dignum*

(sayes he) *si oleum majoribus inderetur abenis in quibus Liquor primus, Caldo dictus, purificatur, saccharo conficiendo plane foret ineptus: vicissim si minoribus lixivium sicut majoribus in-*

fundatur, æquè impossibile saccharum conficere. So much the Fluidity and Firmness of bodies depend upon their texture, how much soever Chymists would have them depend upon Salt.

But to this borrow'd Observation, though borrow'd of Authors not to be distrusted, we will adde two or three Experiments of our own, which we hope may the more confirm the Doctrine by us propos'd touching Stability in Bodies, because it was our aim in them to bring light by them to the matters we treat of.

First then, we prepar'd a Liquor elsewhere to be describ'd, which is almost if not altogether as saline as *Aqua fortis* it self, or any other acid Spirit that is commonly known: and yet when in this Liquor we laid fragments of solid Harts-horn of several sizes to steep, ev'n in a cold place, the *Menstruum* was so far from hardning them, that it would (without dissolving them as corrosive Liquors do metals) gently pierce into them and soften them, so that

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in about two or three dayes it would reduce them to a kind of white slime or mucilaginous substance at the bottom of the Liquor. We took also good salt of Tartar, and on it pour'd good Spirit of Vineger, as long as the affusion of it would produce any ebullition: Then we distill'd off the Liquor, which came over almost insipid, the saline parts that make Spirit of Vineger so sharp, being retain'd by the Salt of Tartar: Upon the remaining dry mixture we pour'd fresh Spirit of Vineger as long as any hissing ensu'd thereupon, and afterwards abstracted the aqueous parts of this parcel of Liquor also, and so we proceeded, till having sufficiently impregnated the fix'd Salt with the saline parts of the distill'd Vineger, we obtain'd according to our desire a mixture which (though it were all made up of Salts, and such Salts too as being made by the Chymical Analysis of the Bodies whence they were drawn, may according to the Chymical Doctrine be look'd upon as pure and Elementary) was yet so neer Fluidity, that it requir'd not the heat of the fire to turn it presently into a Liquor, which shape it assum'd with a gentler warmth than one would expect from a saline Body. Lastly, we took common Oyl of Vitriol, and cast into it diverse little pieces of Camphire, which floating upon it were by degrees and after some hours wholly reduc'd into a reddish Oyl, that was to be seen altogether upon the top of the other Liquor. Then having formerly try'd that Oyl of Vitriol would easily mix with common Oyl, we try'd also by shaking the saline and Camphorate Liquors together to unite them, and easily confounded them into one high-colour'd Liquor, which seem'd very uniform, and continu'd so (at least as to sense) for many hours. Then we added to this mixture three or four times as much fair water, and (as we expected) the Camphire immediately recover'd a white consistent Body, and by degrees settled at the top of the Liquor: where we may observe, that the Camphire is not made hard but fluid

by its mixture with the saline Corpuscles of Oyl of Vitriol, and exchanges its Fluidity for Firmness upon the affusion of Saltless water. And thus much it may suffice to have said touching the Chymists deriving the stability of Bodies from their abounding in Salt.

And as for the hardness and brittleness they ascribe to the same principle, how much they may be increas'd or diminished in a body without the accession or decrement of the saline principle or ingredient, may appear by that Experiment mention'd by us to several purposes, of tempering a slender piece of Steel; for when it has been sufficiently heated, by plunging it red hot into fair water, which is more likely to dissolve than increase its Salt, you may make it not very hard alone but very brittle, whereas by only suffering it to cool leisurely in the air, it will be both much less hard and more tough, and if after having quench'd it in cold water you again heat it till it have attain'd a deep blew, it will become (comparatively) soft and very flexible, and that not from any wasting of the saline ingredient by the fire, for if this softn'd steel be again heated red hot and suddenly refrigerated, whether in water or otherwise, as before, it will re-acquire both hardness and brittleness.

Now that by these operations a real change is made in the disposition of the small parts of the steel, we have elsewhere evinc'd ev'n by a sensible proof. And that by procuring a closer order & more immediate contact of the parts of a body, a man may without encreasing the Salt encrease the hardness of it, is, as we formerly also noted, obvious in Snow, which whilst it lies in flakes as it falls upon the ground, composes but a soft and yielding body: But when the same snow is by being strongly press'd every way betwixt the hands formed into Balls, the little whether Iceicles or frozen bubbles it consists of are so approach'd to one another, and forced into an order which allows so little

wast of room, that the formerly-intercepted spaces being most of them fill'd up with little bodies, the Iceicles can no longer yield as they did before to the pressure of a mans fingers, but constitute a mass considerably hard, which yet may be made harder being melted into water, and afterwards frozen into Ice ; for this having been a fluid Body, (and in such, Room is wont to be better husbanded than in others) the bubbles intercepted in it cannot keep it from being of so close a texture as to be considerably hard.

I know that not only profest Chymists, but other persons who are deservedly rank'd amongst the modern Philosophers, do with much confidence entirely ascribe the induration, and especially the Lapidescence of bodies to a certain secret internal principle, by some of them call'd a form, and by others a petrifying Spirit, lurking for the most part in some liquid vehicle. And for my part, having had the opportunity to be in a place where I could in a dry mould and a very elevated piece of ground cause to be digg'd out several Crystalline bodies, whose smooth sides and Angles were as exquisitely figur'd as if they had been wrought by a skilful Artist at cutting of pretious Stones, and having also had the opportunity to consider divers other exactly or regularly shap'd Stones and other Minerals, some digg'd out of the Earth by my friends, and some yet growing upon stones newly torn from the Rock, I am very forward to grant, that (as I elsewhere intimate) it is a plastick Principle implanted by the most wise Creator in certain parcels of matter, that does produce in such concretions as well the hard consistence as the determinate figure. We deny not then, that these effects depend most commonly upon an internal principle, but the difficulty consists in conceiving how that internal principle produces its effects, which these Writers not pretending to explicate intelligibly, we thought it not amiss briefly to survey some of the

principal ways by which it seems that Nature makes bodies firm and stable, whereby we may be assisted to judge whether it be as necessary to have recourse to a plastick Principle or a Gorgonick spirit in all other quick and notable Indurations of Bodies in the cold, as in the hardning of such Bodies whose curious and determinate either internal Textures or outward shapes (common to several Concretions of one kind) argue their having been fram'd by some one formative power, or by diverse seminal Principles conven'd together. But this we will do without affirming either that she cannot by some other yet unobserv'd way make consistent bodies, or that of the ways by us discours'd of, she is wont so to confine her self to any one, that she does not frequently make use of two or more of them to produce the same effect.

And because Hardness is a high degree of Firmness, I suppose it will not be impertinent to shew by some examples how small an external operation may without any appearing adventitious Salt make a soft body hard, and even brittle, when there appears not any other change to be made than that of the Texture or disposition of its component particles.

It is a Tradition amongst Naturalists, that Coral grows soft at the bottom of the Sea, but when it is brought up into the open Air, though it retains its bulk and figure, it hardens into a stony Concretion, according to that of *Ovid*.

Ovid. 15: Sic & corallium quo primum contigit auras
Metamorph. Tempore durescit, mollis fuit herba sub undis.

Whether or no this Tradition is strictly true, we had not opportunity when we staid at *Marseilles* (whose neighbouring Sea is the chiefest in *Europe* where Coral is wont to be fish'd) to give our selves an ocular satisfaction. But what-
 even:

ever some say to discredit the tradition, nay, how confidently soever *Beguinus* (who seems to have the most strongly argu'd against it) hath rejected it, it must not be denyed to be, sometimes at least true, (and that's enough to serve our present turn.) For the Learned *Gassendus* in the Life of *Piereskus*, relating how that incomparable Gentleman had the curiosity to fish for Coral near *Toulon*, (a noble Port not far from *Marseilles*) has among other things this passage, (*viz.*) The plants which were pluckt up and drawn out were neither red nor handsome till their Bark was pull'd off; in some parts they were soft, and would give way to the hand, as towards the tops, which being broken and squeez'd they sent forth milk, like that of Figs. I remember likewise, that the Learned Jesuite *Fournier*, who being also a French Hydrographer, and one that writes of *Marseilles* and *Toulon* as places very well known unto him, may be safely credited on this occasion, after he has particularly describ'd the way of fishing Corals near *Toulon*, he adds, These plants are neither red nor polish'd when they are drawn out of the water, till their Rind have been taken off, nay, they are soft, and being broken or else squeez'd betwixt the fingers, they throw out a kind of milk resembling that of Figs; and when one leaves off pressing them, he may see the small holes or pores that harbour'd the milk that was squeez'd out. Thus far He. The credibleness of a good part of these narratives has been confirmed to me by a practiser of Physick in the East-Indies, who having made some stay at his return on the Island of *Mehila*, (near that of *Madagascar*) where store of white Coral is reported to grow, I enquired of him whether he had gathered any, and whether he found it soft whilst it was growing? and he answer'd me, that he had.

Beguin. Tyrocin.
Chym. Lib. 2^{do}
Cap. 10.

Lib. 4^o. Anno Do-
mini, 1624.

Hydrograph. de P.
Fournier. Lib. 4.
Cap. 27.

had of late years diverse times gather'd Coral upon the Sands of that Island, and found it, when he gather'd it, exceeding white, and (to use his expression) as soft as an Onion, adding, that though it would in a very short time grow hard in the air, (which he ascrib'd, how justly I know not, to the external heat of the Sun) yet it is very well known to the Sea-men that deal in that ware, that if it be not gather'd at a seasonable time of the the year it will not keep long, but either crumble away or otherwise decay, which disagrees not with the experienc'd

Lib. 4. Cap. 68. *Piso*, who in his natural History of *Brasil*, speaking of some places of the *Brasilian* Coast, where diverse stony plants, some like little Trees, some otherwise fram'd, may be seen in clear weather growing in the bottom of the Sea, tells us, that, *è fundo eruta mox durissima, si insolentur in littore, sicca niveique coloris fiunt.* As remarkable a change is that I meet with in *Scaliger*, who tells us as upon his own knowledge of some, who at the Urinary passages voided a slimy matter, which in the Air coagulated into a firm substance; the story being memorable, take it in his own words thus: *Ex bovillis oppidanus nostris adjutus medicamentis eminxit vitrum sane ex illa nobili Paxagoræ pituita, dum mingeretur albuminis mollitie emissum vitri duritie ac splendore, Senatoris filius ejecit, pultis modo multos, & maximos: qui aeris contactu postea in gypseam tum speciem tum firmitatem concrevere; hic quoque nunc rectè valet.* Having likewise had the acquaintance of an inquisitive Merchant of *Dantzick*, and also of an ingenious Chymist, that spent some time in that City and the neighbouring Country, along whose coast our European Amber is wont to be dragg'd out of the Sea, I enquir'd of them, whether they had never observ'd in Amber a property like that which is reported of Coral: and one of them, as I remember the other also, hath assured me upon his own particular Observation, that lumps of
of

of Amber are sometimes taken soft out of the Sea, and grow hard in the Air; which is the more credible to me, because I have at a *Polonian* noble-mans seen (besides other intercepted things) a whole Spider, and that none of the least, perfectly inclosed in a piece of hard and transparent yellow Amber. And elsewhere I have seen ten or twelve (if I mis-remember not the number) pieces of such Amber, which contain'd, one a Fly; another a Spider, a third a Straw, and each of the rest some such other thing. And it seems not impossible, that the contract of the external air may put the parts of such small Bodies into a new motion, whereby some volable Corpuscles that hinder their reciprocal adhesion may be excluded, and the particles themselves prest or otherwise dispos'd into a closer order; and we find that some Oyl-colours, after they are brought to their due temper, may be preserv'd very long in the same degree of softness, if they and the shells that contain them be kept all the while under water, whereas in the air they would quickly change their Texture, and become dry and hard.

But though in this last mention'd Example, and some others the removal of the body out of the water into the air seem manifestly to contribute to its growing hard, yet it seems not to us so easie to determine what share the air has in effecting such indurations: for *Gassendus* relates of *Piereskius*, that he being wont in the Summer time to wash himself in one of the lesser streams of the River of *Rhosne*, he there made the following Observation. Once upon a time he felt the ground, which he had wont to find even and soft, to be grown hard with little round balls or bunches, like hard boiled Eggs *Gassend. in vita Piereskii, lib. 1.* when the shell is peel'd off; at which wondering, he took some of them up, and carried them home, that he might shew them to his Master, and demand of him the Reason. But the miracle was increas'd when a few

few days after returning to the River, he found those little balls or lumps turned into perfect pebble stones, which he observ'd likewise to befall those which he had carried and laid up at home. But how far this story will prove that such coagulations must be effected by a substantial form or a petrifying Liquor, we define not, especially since, not to repeat what we deliver'd already touching calcin'd Marble out of *Fournier*, we have elsewhere deliver'd upon our own Observation, that two or three spoonfuls of such pap of burnt Alabaster as we have lately been speaking of, (and instead of which Artificers use another stone call'd by them Plaster of *Paris*, burnt and temper'd up with fair water) did in the bottom of a vessel-full of water into which we pour'd it in a short time coagulate into a hard lump, notwithstanding the water that surrounded it; which, it seems by the Texture of the mass, was kept out of its pores, as it is out of those of the Oyls of Cinnamon and Cloves, which though fluid bodies, and sinking in water, suffer not its particles to insinuate themselves into theirs: and Artificers observe, that the coagulating property of burnt Alabaster will be very much impair'd, if not lost, if the powder be kept too long, especially in the open Air, before it be made use of; and when it has been once temper'd with water and suffer'd to grow hard, they tell me they cannot by any burning or powdering of it again make it near so serviceable for their purpose as before; so much doth the coagulation of these powders mixt with water seem to depend upon their Texture and other Mechanical qualities.

I remember also, that though the bones found in the Hearts of Deer, and so magnified by Physicians, do in the air acquire a hard and bony consistence; yet having had the curiosity to consider one of them in the Heart of a Deer newly kill'd, I found it there of a cartilaginous softness and flexibility.

And

And here I will adventure further to confess, that I have oftentimes doubted whether or no not only consistent Bodies but some of the most solid ones in the World may not have been fluid in the form either of Steams or Liquors, before their coalition and their concretion either into stones or other mineral Bodies. I know there are many who think that Stones, Marchasites, and other such solid and durable Bodies, were made together at the Creation or other beginning of the Universe, and who will not admit that such concretions can be now generated. But not here to debate that famous Controversie, whether stones may be said to grow and to be nourish'd, in the strict sense of those Expressions, I think it not difficult to shew that such parcels of matter are now to be met with in the form of stones as did not before appear under that form, but whilst it was divided into minute parts either was it self some fluid Body or other, or at least did as a material part concur to the constituting of one that was so. Of this, besides what we elsewhere deliver concerning it, we shall anon have occasion to mention some proofs; and therefore we shall now only mention two or three instances. The first whereof shall be, that we saw, among the Rarities of a Person exceedingly curious of them, a stone flat on the outside, on one of whose internal Surfaces was most lively engraven the Figure of a small Fish, with all the Finns, Scales, &c. which was affirmed to have been enclosed in the Body of that stone, and to have been accidentally discover'd, when the stone chancing to receive a rude knock upon its edge, split asunder. I remember also that a while since a House-keeper of mine in the Country inform'd me, that whilst a little before he caus'd in my absence one of my Walls to be repair'd, the Mason I was wont to employ casually breaking a stone to make use of it about the Building, found in it (to his wonder) a piece of Wood that

seem'd part of the branch of some Tree, and consequently was afterwards enclos'd with that solid case wherein he found it. This cavity in the body of the stone and, as I remember, the stick it self he took out of it, he forthwith brought my House-keeper, to whom I have given directions to send them me. For this example seems to me a more cogent proof of the increase of stones, than some others that eminent Naturalists much rely on, for Reasons

*In some of the
Authors Papers
about the Ori-
gine of Mine-
rals.*

discours'd of in another place: where we also make particular mention of that Ghur or Metalline juice, which though the Latin Writers of Chymical and ev'n of Metalline matters have not, that I remember, given us any account of, yet I find a German or two, that were very conversant in the Mines themselves, to have in Books written in their own Language taken a special notice of it. Besides, I have at present something to deliver upon my own Observation, which unless we will suppose (what seems not probable) that there were from the beginning made together with and in the midst of great Masses of one kind of Mineral little parcels of another of a quite differing sort, seems manifestly enough to argue, that either whole quarries of stone, or heavy and shining Minerals, or both, may have been fluid Bodies. The Observation whereon I ground this Conjecture is, not only that we have met with in Lead-ore and also in *Minera Antimonii* parcels of a white stone or spar environ'd with a Metalline body, though I think I have yet by me such lumps of Ore; but chiefly that I have with my own hands taken a hard and ponderous shining Mineral, which I keep for a Rarity, like a Marchasite, of the shape of a Pear, and of about the bigness of a Walnut, out of the very Body of a stone wherein I suspected it to be enclos'd, and which environ'd it on all sides: and this I took not out of a small and loose stone, but a large stone digg'd out to make Sta-
tues

tues of. And I remember that one of those that wrought upon it told me, that in fashioning it into Statues they found some more Minerals in the same parcel of stone, which were also presented me. To which I shall adde, that an ingenious Statuary having in another place taken much pains to saw asunder a very large stone, when he came to the midst of it, found he could saw on no farther, and the stone being afterwards broken, he perceiv'd that that which so obstinately resisted his Saws was a round Marchasite, which he brought to me, as a Lover of such Curiosities. But I made him for my further satisfaction bring me also that part of the stone wherein the Marchasite stuck, and by comparing them together discern'd that as much of the stone as was contiguous to the Marchasite had a kind of rust about it, and fitted the Marchasite so close, as if either the Marchasite had been formerly liquid, and had afterwards been as it were moulded in that Receptacle, or the stone had been formerly of some soft or fluid matter, which did exactly accommodate it self to the shape of the other Body; or else, as if both the matter of the stone and that of the Marchasite had been at once fluid Bodies, but had each of them preserv'd its own surface distinct (according to what we formerly noted of differing fluids) till one of them (probably the Marchasite) first growing hard, the other, as being yet of a more yielding consistence, accommodated it self to the harder's figure.

But the most eminent Instances to declare how much the Fluidity and Firmness of Bodies depend upon the contrivance and Texture of their parts, are afforded us by those waters which being permitted to rest a while do spontaneously cease to be fluid and coagulate into stone it self. There was lately an Ingenious Man, who going to visit some Leaden Mines wherein he had a share, found in the Mountain in whose Entrals they were hid a Cave, from whose arched Roof there drop'd down a petrescent Liquor,

which oft-times before it could fall to the ground congeal'd, and by apposition of like matter increas'd so much that they hung from the Roof like Ice-icles in a frosty night from the sides of a House; and of these he gather'd and brought me diverse, which are perfect stones hard and brittle, and of eight or ten inches long, and proportionably thick. Another ingenious friend of mine being lately in *France* in the Cave so famous for petrifying Liquor to be there seen, observing some drops of water to congeal into stone whilst he stood by, took them away with him, and sent them me in a Letter. Nay, we shall scarce deny that an external agent of almost insensible bulk may turn animal Bodies into stony ones, by introducing a new texture into their parts, if we will with some modern Writers believe *Aventinus*, who in his *Bavarian* History has recorded, that at a time and place by him specified, above forty Country-men, as also some Milk-maids with their Cows kill'd upon an Earthquake, had their Bodies by a terrene Spirit turned into statues, which he says were seen by the Chancellour of *Austria* and himself. And some relations of this Nature we meet with in other Authors, which, if they be allowed of, seem much to confirm our Doctrine; for in these strange petrifications, the hardning of the Bodies seems to be effected principally, if not only, as in the induration of the fluid substances of an Egg into a Chick, by altering the disposition of their parts, since the petrifying wind or steam cannot be suppos'd to have any such considerable (perhaps not any sensible) proportion as to bulk to the body chang'd by it, as to be thought to effect this change principally as an Ingredient.

Adde we to all these things, that *Pamphilio Piacentino* is by another Author quoted for writing an unparrel'd Story, which because written in Italian, I shall English the substance of it, which is this: That a Woman in *Venice*, after having eaten an Apple, was taken with hideous tortures, and

and in the space of twenty four hours dying, was turned into exceeding hard stone, and this was judged to be the effect of the poyson'd Apple she had eaten. Which narrative, if we may believe it as confidently as the famous Alleger of it *Pamphilio* appears to do, would seem to argue, that event to the wonderful induration of Bodies there is sometimes no other principle requisite than what may result from the lucky mixture of the parts of several Bodies. And lest we should seem to build altogether upon the Observations of others, which cannot by us be now brought to strict examination, we will have recourse to a practicable Experiment of our own trying, which though we have elsewhere mention'd, we shall not scruple here to repeat, because we there omitted to speak of that Circumstance of it, which is the most pertinent to our present design.

Take then two Ounces of Quick-silver, two Ounces and a half of the best Verdigreese, about half an Ounce or an Ounce of common Salt, a pint or pound of White-wine-Vinegar, and as much fair water, mingle the Verdigreese, Quick-silver, and the Salt very well, and put the mixture with a little of the Vinegar and water into a new Frying-Pan, (I try'd it in a new Earthen Vessel; but without good success) in which fry it over the fire for diverse hours, keeping it continually stir'd, and putting in more Vinegar and water from time to time, as that already put in consumes away; then take out the mixture, and in several clean waters wash it carefully from the adhering Salts; then dry away all the Aqueous moisture with a clean linnen Cloth, and you shall have a bright Amalgama almost like Quick-silver. Now that which is remarkable and to our present purpose in this Experiment is, that though this dry'd mixture be a good while after it is perfectly cold not only soft, but so neer to fluid, that I have cast it into moulds and made imbest Images of it, (when it has been dexterously made, but scarce otherwise;) I have found, that by laying it a few
hours.

hours in the air, which seem'd less cold than it self, it has acquir'd such a hardness, that being thrown against the floor it would rebound, and was brittle like over-harden'd Steel. And yet in this Example the induration of the Amalgam appears not to proceed from an innate and inward principle, but from the new Texture resulting from the coalition of the mingled Ingredients that make up the Amalgam, whose parts being variously moved, partly by the fire (and perhaps too by the Salts) and partly by the native propensity to motion of the Mercurial Corpuscles, were by little and little, or by degrees, so dispos'd, that whereas before touching one another but loosely, it was easie to thrust some of them towards the middle of the body without stirring much of the Mass (as to sense) by this change of Texture the particles are brought to touch one another more closely and in greater portions of their surfaces, and to be so complicated, intangled, or otherwise connected among themselves, that you cannot endeavour to thrust one of them out of its place, but that its motion shall be resisted by many others, to whom it is so fasten'd, that you cannot move one part of the Mass without either moving the whole with it, or manifestly breaking it off from the whole, and thereby destroying the continuity and unity of the Body.

Now whereas in setting down this Experiment, we spoke as if several Ingredients did concur to constitute the soft Mass, which afterwards grew so hard, we might very safely do so, since the Quicksilver was not so barely chang'd in Texture as that formerly said to have been coagulated by the meer fume of Lead, but conceal'd in its self a great number of metalline Corpuscles besides others, as we made appear by separating from the Amalgam, meerly by the force of fire, a pretty quantity of true and perfect Copper. That the Salts also both were Ingredients (though in small proportion) of the Mass, and might have some opera-

operation upon the other particles, we may render probable by this, that having purposely expos'd some of this Mass for a pretty while to a moist Air, we found, as we look'd for, that the formerly invisible particles of Salt, that had so insinuated themselves into the Amalgam, that all the water wherein it was wash'd did not separate them from it, had so wrought upon the metalline particles that were most outward, that they had in many parts of the surface of the Mass turned themselves with it into a kind of Verdigreese, which seem'd almost to hide the surface of the Concretion. And that in the more inward parts of a much harder Body than our yielding Amalgam, where Cuprious particles abound, saline Corpuscles may have a great operation, may appear by certain sorts of Minerals to be found in some parts of *England* and elsewhere under the form of stones, of which they make Vitriol; for these abounding with vitriolate, that is, both saline and metalline particles, will, after they are taken out of the ground and laid in the open air, by the working of the inward Salt, some sooner and some later, swell and burst asunder, which could hardly come to pass without a great change made in the internal disposition of the parts, of such stony Concretions. And I remember, that having laid a mineral of Kin to these stones a while in the air, though but in a Chamber, I found its surface powder'd with little grains of Vitriol, as both their Colour and their Taste inform'd me.

Now whether or no we suppose that the fire did put the parts of the Amalgama into any lasting Agitation, yet the Mass being almost fluid after it was taken from the fire, its parts may according to our notion of Fluidity be well suppos'd to have some kind of motion among themselves; and it will not be deni'd, that the fire might concur with other things to make that motion convenient to cause the parts to fasten themselves to one another: For that the motion
wherein

wherein a soft and almost fluid Body is once put may possibly tend to harden it long after that motion seems to be extinct, may be made probable by what has been affirm'd to me by eminent and experienced Masons, namely that the best sort of Lime made into Mortar will not have attain'd its utmost compactness till twenty five or thirty years (perhaps not till three or fourscore) after it has been employ'd in Building; and this is given me as one of the Reasons, why in the demolishing of antient Fabricks, it is sometimes more easie to break the stone than the Mortar.

And lastly, that we also made mention of the Texture resulting from the mingled Ingredients of our Amalgam, we might justifie by saying, that having changed the proportion of the Quick-silver to the Verdigreese, we found that the Amalgam coagulated much more slowly, and when it was coagulated, was much less hard than when one used the quantities above specify'd.

Here I should put a period for the present to this Discourse, but that having in a late Writer met with a notable Observation of the natural Induration of a soft Body, I think it worthy to be here annex'd, partly, because the French Book is not common, no more than the Observation; and partly, that by conferring together this natural Induration with that Artificial one freshly mention'd, it may the better appear how Nature and Art have sometimes resembling operations in rendring Bodies solid. My

Author then (by name *Pierre Pelleprat*) being not long since sent with some other Jesuites upon the laudable errand of Preaching the Gospel to the Indians of the Southern *America*, has among other things this passage in the short Relation he makes of the American Continent. There is (says he) one thing worthy of Observation neer the mouth of this great river (he speaks of that of the *Amazons*) which

is,

*Relation des Mes-
sions des P.P.F.F.
secon. part, cap. 1.*

is, That men find there a kind of green Clay that is soft as long as 'tis in the water, so that one may print on it all kind of Figures, and give it what shape one pleases; but when it is expos'd to the Air, it hardens to that degree that Diamonds are not much harder than the stones it affords ----- I have (adds he) seen Hatchets made of this Clay, which the Savages employ'd to cut Wood with, when they had not the use of ours, &c.

And now at last, I see 'tis time to put a period to a discourse, that has been unawares lengthned far too much already: But yet I think you will easily pardon me, if I conclude it not abruptly; but with the recital of an Experiment, which having had the honour to be seen, as to the main part of it, by an illustrious meeting of Curious Men; their having been pleas'd to speak very advantageously of it to others, excited a curiosity among them, to know by what artifice, effects that were so uncommon, had been produc'd. The Scope therefore, and the manner of making the Experiment, were in short as follows.

Being desirous to shew how much Fluidity and Firmness may depend upon the Texture and upon the Motion or Rest of the insensible parts of Bodies, I first make with good Spirit of Vinegar, a Solution of Coral so strong, that when 'tis filtrated and cool'd, it will commonly, after some time, begin to have a kind of Sediment at the bottom; the clear Liquor I gently pour off, when the Experiment is to be made, and to this I put a convenient proportion of very well dephlegm'd Spirit of Wine, which if it be pour'd on very slowly and warily, may be made for a 'pretty while to swim upon it in the form of a distinct Liquor: but if by a few shakes I mingle them together, they will presently unite into a Concretion, of which when the Experiment succeed very well (as it did when I shew'd it to the above-mention'd Assembly) not

one drop will fall to the ground, upon turning up the wide mouth Glass it should be made in, and holding it with the mouth directly downwards.

And this so hastily produc'd consistence may be durable enough, if it be carefully lookt to: But to dispatch the whole Experiment in a short time, I take a little strong Spirit of Nitre (which perhaps is not needful if good Aqua Fortis be at hand) and putting about an equal, or other convenient quantity of it to this Mixture, I nimbly stir it and the Spirit together: whereupon the Whole is reduced in a very few minutes to a transparent Liquor.

N. B. Though I have divers times made and shewn this Experiment, yet there are so many Circumstances requisite to make the first part of it succeed very well (for to make it succeed in some measure is not so difficult) that the event has sometimes deceiv'd me, in spite of the several Tryals I have made. Wherefore 'twill not be amiss to intimate.

First, That one of the first times, if not the first, I made such an odd Concretion, was, with the Solution not made with Spirit of Vinegar, but with Spirit of Verdigrease: (which I commonly distil without additament) though afterwards I was invited to prefer strong Spirit of Vinegar, which was the Liquor wherewith the recited Experiment was exhibited.

Secondly, That it often happens that if the Solution of Coral (which is not the only body wherewith I have made such Tryals with indifferent good success) be not sufficiently strong and impregnated) with Saline parts, or the Spirit of Wine be not sufficiently rectify'd, the shaking of the two Liquors will not change the consistence of the whole mixture, but leaves some part of it fluid, or else the Concretion will not begin presently to be made, but require to be waited for a while.

Thirdly, That I once at least (if not oftner) observ'd,
that

that when by mingling the two Liquors and shaking them in a narrow mouth'd Glass whose Orifice was stoppt, they would not concoagulate (as it was confidently expected they should) yet by trying the Experiment in a wide mouth Glass to which the Air had free access, it succeeded to my content.

Fourthly, That in the Reduction of the Concretion to a fluid Body, 'tis not proper to employ in stirring it a Knife or any other Metalline Body except it be of Gold; but rather some Stick of Glass, or at least some clean Stick of Wood, lest the *Menstruum* should corrode it, and thereby spoil, or at least blemish the Experiment.

Fifthly, That the proportion betwixt the Coralline Solution and the Spirit of Wine depends so much upon the strength of the former Liquor, and the dephlegmedness of the latter, that 'tis scarce possible to determine generally and exactly what quantity of each ought to be taken; and therefore a Tryal or two made with a little of the particular Solution you intend to employ (for some Solutions require more, others less Spirit of Wine to concoagulate adequately with them) will better direct you what proportion of Spirit will sute that particular parcel of Liquor than any general Rule I can propose.

I know I might here, and perhaps it may be expected that I should, take an occasion to treat also of Hardness, Softness, Brittleness, Toughness, Stiffness, and those other qualities that are of kin to Fluidity and Firmness; but though I confess, I once had thoughts of writing a kind of History of more Qualities than those, yet remembering that wise Counsel given us by one of the Antients, *Noscenda est mensura sui*, I am now very well content, after having already tir'd my self, and I fear you, to recommend so useful but difficult a work to Persons more able and more at leisure than I find my self to go through with so great an undertaking; contenting my self at present, to have attempted

in what has been delivered concerning a couple of qualities of such extent, that every sensibly big Body in the Universe seems indow'd with one or other of them (I mean concerning Fluidity and Firmness) the explicating of Qualities somewhat more intelligibly than is wont to be done in the Peripatetick Schools, and to have open'd a way (which I hope many will tread) of applying Chymical Observations and Experiments to the deduction of those effects of Qualities from such general and obvious affections of matter; as Bigness, Motion, and Figure, which even the Hermetical Writers have hitherto contented themselves to refer to Salt, Sulphur, Mercury, and the like: the Chymical notion of which (three Principles) though of very good use in some other (especially of the more practical) parts of Physiology, seems not as yet to have brought any great light to such matters as we have been treating of, having been hitherto directed not so much to the indagation of Causes, as to the production of Effects.

The End of the Notes touching Fluidity and Firmness.

F I N I S.

OF

Absolute Rest

IN

BODIES.

LONDON,

Printed for *Henry Herringman* at the *Blew Anchor* in the
Lower Walk of the *New-Exchange*, MDC LXIX.

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Absolute Rest

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BODIES

LONDON

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AN

ADVERTISEMENT.

S*Ince it hath not been thought amiss that something should be intimated to the Reader about the Occasion of the ensuing Tract, I shall acquaint him with it as briefly as I can, by telling him that it was This. Some very ingenious Gentlemen hapning to meet as Visitants at the Author's Lodgings, fell accidentally into a Discourse about the Absolute Rest supposed to be in many Bodies, that seem'd to have its Rise from a mistake of the true meaning of a passage or two in the History of Fluidity and Firmness, (that was then re-printing.) But the Conference chancing to have a period put to it, whilst several things pertinent to the Author's purpose remained yet unsaid: the Curiousness of the Subject invited him to draw up (hastily enough) the Sum of what he had said, and might further have said if opportunity had serv'd, about the Point in debate, for the further satisfaction of an inquisitive Virtuoso that was present at it. And this was the Rise of the following Discourse, which being written on an Occasion administred by the History of Fluidity and Firmness, whereof a New Edition was ready to come abroad; 'twas thought not improper that this Tract should attend it, as a kind of Appendix, without the First and Last part of a Letter, whereof the Body only is necessary to the Design of it.*

AN

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1771

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(I)



AN
E S S A Y
Of the
I N T E S T I N E M O T I O N S
Of the
P A R T I C L E S
O F
Q U I E S C E N T S O L I D S.

Where the Absolute Rest of Bodies is called
in Question:

S E C T. I.

TO remove the Doubt or Scruple that began to be discoursed of just before we last parted, I shall need to do little more than enlarge the Particulars, which (you know) I had then time but briefly to make mention of. For the state of the Question was, (as you may remember) this, *Whether there be among Bodies*

B

any

any absolute Rest? On which occasion I answered, That Rest being a word that to me seem'd somewhat ambiguous, I thought 'twas requisite to clear the sense of the Question before I offer'd at answering it.

For the word Rest, when we speak of distinct Masses of Matter, lookt upon as quiescent, does in the vulgar acception of the Term signifie, such a state of a visible and entire Body, or (rather) of the Corpuscles it consists of, that they are actually *Unmoved as to sight*; the Eye (and perhaps not the Touch) being not able to discern any local Motion in them.

Consonantly to this first Member of the distinction of the word *Rest*, I briefly intimated to the Company, that in this sense of a Corpuscles being at Rest, I thought it manifest, that there is such a thing in *Rerum Natura*: Since without granting such a Rest in the component Particles of some kind of Bodies, as Diamonds, Iron, Porphiry, &c. 'twill be (I conceive) very hard to explain, how there can be such solid Masses (as those Minerals are) made up of small and separable Particles. Which being said, I added, that I saw no reason why such a kind of firmness, where the inward motion of the insensible Particles is almost infinitely slow, may not suffice to give an account of as great a firmness as we use really to find among consistent Bodies.

But whereas I had intimated to the Company, by the lately begun Distinction, that besides this popular sense of the word Rest, there was a second, more rigid and Philosophical Notion, or kind of Rest, which for distinction sake may be called Absolute or Perfect Rest; which imports a continuance of a Body in the same place *precisely*, and includes an absolute Negation of all local Motion, though never so slow or imperceptible; I told them that in this rigid sense of the word Rest, I durst not affirm, that there are any Bodies at Rest in the

Universe (at least for any long time) but willingly allowed it to be made a Problem, whether there be any or no: adding, that perhaps I enclin'd to the Negative part of the Question.

Having thus historically summ'd up what pass'd betwixt us about the state of the Controversie, I need not tell you, that the Doubt I express'd was thought to relish too much of a Paradox; and therefore since the company's quick separation allow'd me then no opportunity of enlarging, and since I promis'd no better Arguments than might be expected in a point that I propos'd but as Problematical; I shall now endeavour to shew you that the side of the Problem I was judg'd enclin'd to, is (at least) not so improbable as some thought it.

To prove Negatives directly, being wont (as you well know) to be no easie Task, and especially in such cases as this; you will not, I presume, expect that I should attempt the proving of my Conjecture otherwise than by shewing positively, that some of those Bodies which we think to have their parts most at Rest, are not exempted from having Intestine Motions in them; since 'twill be consequent to such a proof, that it must be probable, that in other Bodies whose Solidity is confessed to be inferiour, the component Particles are not in a state of Perfect Rest.

S E C T. II.

If it were necessary and expedient, I should begin my Arguments by saying something against Absolute Rest, in favour of the contrary Opinion, by arguing, *à Priori*, as they speak, from the constitution of the World, whether we consider it according to the Epicurean or the Cartesian Hypothesis of the Origine of things. For the Epicureans supposing this World to be produced

by the casual concurrence of Atoms, and ascribing to every particular Atom an innate, and unlooseable mobility, or rather, an actual motion, or a restless endeavour after it; 'tis consonant to think, that though in Concretions, they so entangle one another, that they cannot in a short time, or a visible manner clear themselves from one another, yet they do incessantly strive to dis-entangle themselves and get away: by which means there is always in the Atoms even of Solid Bodies, actual endeavours of each of the distinct Atoms to extricate it self from the rest: (which endeavours usually at last succeed, whence comes the decay and destruction of Bodies) and in the mean while these perpetual and contrary endeavours produce intestine Commotions in the internal parts of the Body wherein these Atoms were imprison'd.

On the other side according to the Cartesian Doctrine, the *Materia Subtilis*, that constantly passes like a stream through the Pores even of the solidest Bodies, may well be suppos'd in its passage to be continually shaking or otherwise agitating the insensible Particles that make up the body that seems to be at Rest, without discovering their Motion to the Eye: As when in Summer time (to explain my self by a Comparison) a gentle breath of Wind passes through a Grove removed a pretty way off from the Spectator, though his Eye discern no change in the Grove he looks on, yet the Wind as it blows through the Trees will shake some of the Branches as well as the flexible Twigs; and not only blow the Leaves into various postures, but blow some of them quite away.

I might easily enlarge on this Subject, but having elsewhere done it on another occasion, I think it may be now more proper to satisfy some of the Company, who are yet entangled with the same prejudice with many
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other very Learned men, who look upon it as a Precarious and Chimærical Fancy of the Atomists, to imagine, that in Solid, and as to sense, Quiescent Bodies, there should be any intestine Motion of the component Particles, neither the Motions nor the Corpuscles themselves being to be seen, and both of them being therefore as well incredible as invisible.

A solemn Debate of the whole Question about the Minuteness of Atoms belongs not to this place, where it may suffice to answer the Objection.

SECT. III.

And first, As I have elsewhere hinted, it may appear by divers of the Phænomena above-cited (in the *History of Fluidity*) that when Water and several other Liquors seem to be continued Masses of Matter, and to be as much at rest, as the very Glasses that contain them; their constituent Corpuscles are in an actual and various though slow and unperceived Motion.

Next, That there may be likewise such a Motion in the minute parts of Silver and Iron themselves, may be easily argued, by heating those Metals till they come to be almost red hot: for then though the eye can discern no motion of the Corpuscles those Metals consist of, yet their being able to burn those that hold them in their naked hands, shews that their brisk Motions may be discovered by the help of the Touch; and if you spit upon them the Liquor will boil, as if it were over the fire. And lest it should be objected, that so anomalous and violent an Agent as the Fire, is necessary to these Tryals, I shall add, that, provided the minute parts be sufficiently agitated, it matters not whether the Motion be produced by Fire or no; for by the nimbly hammering of Iron or Silver, you may put the minute parts into such a Motion, as will make the Metal very hot
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to the Touch, and being communicated to Spittle or Water, will excite Bubbles, and scatter the dissipated parts of that Liquor into the Air, in the form of Smoke or Vapours; nay I elsewhere shew how I have easily excited a very sensible, though not a visible agitation, and heat in the internal parts of a Metal, barely by my naked hands, without any external instrument whatsoever.

And whereas it may be objected, that though the Motion already generated is unseen, yet we may discern a change of the component Corpuscles of a Body which are in the Act of altering its Texture, and introducing a new alteration or quality in the Body to be wrought on, or destroying some pre-existent quality: I briefly answer (for I would not here repeat what I have elsewhere said of this point) by this clear Experiment, that though your Eye can discern no change in the outward and visible, much less in the more latent and internal Corpuscles of Iron: a vigorous Loadstone by passing along its Axis from one Pole of the Stone to the other, and back again, yet the Texture of the Iron is by that action of the Load-stone so changed, that it acquires, and then loses those admirable Qualities we call the Attractive and Directive virtue or faculty peculiar to Magnetical Bodies.

And to shew you that the invisible Motions even of Metalline Bodies may be quick and brisk enough, and may be sensible, though not visible; We shall need to consider but the state of a good Bell so long after the Clapper has struck it, that no shaking or other Motion is to be seen in the body of the Bell it self, and yet it causes in the Air an odd kind of ringing, or if I may so call it, undulating Sound or Motion, which will sometimes last a considerable while; and if the Bell be fitted for sharp notes, 'twill not be without a shrillness: for if
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sounds proceed, as is elsewhere made probable, from the nimble percussions of the Air put into a quick and waving motion by sonorous Bodies; this acuteness of sound will shew, that whilst to the Eye the Bell seems to be at Rest, yet the minute parts of it continue in a very brisk motion, without which they could not strike the air strongly and fast enough to make it produce so shrill a noise in the Ear.

But, I confess to you, that my thoughts present me a Difficulty, which though un-mention'd at our meeting, may afford an Objection, perhaps more difficult than any of (not to say all) the foregoing, namely, That 'tis scarce imaginable, how such solid and hard Bodies should have their internal parts wrought upon by such slight Agents as the air, and perhaps some yet minuter matter that is dispersed in it; and how it is possible, that where there is an actual Motion it should be so slow, that a Corpuscle of Iron, for instance, seated in the internal Part of a Magnetick Needle, should spend so long time as our conjecture requires in travelling so little a space as from thence to the next Superficies of the Needle. But to this double Objection, though some instances which you will meet with in the following part of this Paper, may be properly applyed to solve it: yet not to make your curiosity wait, I will here speak a word or two to each of the members of the Objection.

SECT. IV.

And to the first, I say, That these Intestine Motions of the Corpuscles of hard Bodies, need not be solely, nor perhaps principally ascribed to those obvious external Agents, to which we are wont to refer them, since these may but excite or assist the more principal or internal Causes of the Motions we speak of, as you may gather
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ther from what was but lately mention'd of the connate and unlooseable mobility of the Atoms, according to *Epicurus*, and the permeation of the most Solid Bodies by the Cartesian *Materia subtilis*; and we may see by the sudden effects of the Load-stone, in endowing Steel with Magnetick Qualities, and depriving it of them again (both which suppose the intervention of a change of Texture, and this a production of Local Motion in the Metal) that very minute and insensible Corpuscles of matter are not incapable of effecting durable changes in the solidest Bodies.

And as for the other member of the Objection, I confess it is not easie for us who are wont (perhaps too much) to follow our Eyes for Guides in judging of things corporeal, and to deny existence to most things, to most things whereto Nature has deny'd a visible bulk: 'tis not easie, I say, for us to imagine so great a slowness as 'tis very possible for Nature to make use of in her Operations, though our not being able to discern the motion of a shadow of a Dial-plate, or that of the Index upon a Clock or Watch ought to make us sensible of the incompetency of our eyes to discern some motions of natural Bodies, which reason tells us ought to be incomparably slower than these. But not now to dispute about the existence and Attributes of infinite slowness, or at least a slowness in the next possible degree to infinite: I consider that it has not that I know of been demonstrated, nor attempted to be so, that the motion of the Corpuscle, for example of the Needle above mention'd, must be made in a direct line from the place where 'twas first supposed to be to the Superficies of the Needle; for it seems more rational, and to agree better with the Phænomena, to suppose, that the way of this Corpuscle in the Body 'twould quit, is extremly crooked and intricate (almost like that of a Squib in the air, or on the ground) for it being on the one
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hand urg'd on by the Causes whatever they be that make it strive to fly away, and on the other hand hindred by the Corpuscles whereto 'tis connected, and by the occurrences of other Corpuscles whose motions may be opposite to, or disagreeing with those of our design'd Corpuscle; it may probably, before it can extricate it self, be reduc'd to encounter and wrestle, as it were, with many other Corpuscles, and be by them sometimes thrust or impell'd to the right hand and to the left, and sometimes also repell'd inwards, even after it is come to the superficial part of the Needle; whence it may not presently have the liberty to fly away, but may be drawn back by some other Corpuscle, wherewith it is yet connected, and which happening to be it self thrust inward may draw after it, and so entangle again our almost dis-banded Corpuscle: besides that, the gravity of the component Particles of a Body is oftentimes such, that 'tis easier for the Agent that puts them in motion, to continue them in that slow motion among themselves, than drive them up into so light a *medium* as the air, as experience shews in those Bodies that are called Fixt, as Gold, and Glass, though in actual fusion.

But, I forget that I promis'd you to decline Speculations, and therefore I shall only name to you a couple of Instances which will serve to confirm both what I was lately saying, and what I am now in proving.

SECT. V.

The first of these I shall take from what is usually granted as matter of Fact, namely, that if a Spring, though made of so hard a Body as Steel, be forcibly bent, and kept but a moderate while in that posture, as soon as the force that kept it bent is removed, it will again return to its former Figure; but if it be kept too
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long in that forc't position, it will by degrees lose that which they call the motion of Restitution, and retain its new crooked Figure, though the force that bent it be removed; which shews both the power of some of the more familiar Agents in Nature, and (which is that the shewing whereof I here chiefly aim at) that where there is a continued endeavour of the parts of a Body, to put themselves into another state, yet the motion, or rather the progress may be much more slow than men seem as yet to have taken notice of, since 'twas a great while before the Texture of the Corpuscles of the Steel were so alter'd as to make them lose their former springiness.

But I will second this with a more illustrious Experiment, which will at once confirm what I have just now said, and shew that the Air or the invisible Corpuscles harbour'd in it may have no inconsiderable power to act upon, and effect changes in the solidest Bodies.

To this purpose I shall only observe to you, that *though* if a Bar of Iron having one of its ends held perpendicularly, and at a fit distance, to the Lilly or North-Point of the Mariners Compass (I mean that which points towards the North) it will, as I elsewhere mention, drive it away towards the East or West: and if this same lower end of the Bar of Iron be put into a contrary posture, it will presently lose its temporary magnetism, as I elsewhere declare. Yet if this Bar be very long kept upright in a Window or other convenient place, then, as some late Magnetical Writers will tell you, it will have acquired a constant and durable magnetick power. Which is a Phænomenon that makes exceedingly for our present purpose, since it hence appears both that the Air together with the magnetical Effluvia of the Earth that it receives in its Pores, is able without outward force to work durable changes in so
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solid a Body as Iron, and that the motions of the internal Parts, for these are requisite to the change of the Metal's Texture, are performed with a wonderful slowness, since the Bar must be very long exposed to the air, perhaps before it acquires any durable magnetism at all, but at least before it acquires so vigorous and fixt a magnetism as by this means it may attain to.

But, because most of the Instances to be propos'd in the following part of this Essay, may serve for Confirmations of what we have been discoursing; I shall proceed to them, yet not 'till I have advertis'd you, That I purposely decline to mention divers Phænomena that may even by Learned men be thought fit examples on this occasion, (such as the Nutrition, Growth, and Wasting of Animals and Vegetables) because such Bodies receiving constant supplies of Corpuscles, of several, and often unknown, Natures, there may be difficulties suggested about them, not easie to be cleared without longer Discourses than I can allow this Essay.

SECT. VI.

The first Instance then that I shall mention about Vegetable substances, shall be taken from *Lignum Vitæ*, or *Guaiacum* (for many Artificers give them the same name, and use them promiscuously for the same purposes;) of which, though it seem to be the solidest wood we know (insomuch that I as well as some others have ordinarily us'd it to pound solid Bodies in) yet the skilfullest Tradesmen I have met with, have upon my inquiry informed me, that if it be wrought before it be well season'd by length of time, it will shew it self very frangible; which an eminent Turner told me he had often found to his loss: For having turn'd divers fine pieces of Work of *Lignum Vitæ*, before 'twas duly season'd, he found almost all
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of them by the heat of the Sun (which the uses of many of them requir'd they should sometimes be expos'd to) crack, and cleave asunder, into I know not how many parts; whereby those fine pieces of workmanship were quite spoil'd. And I remember, that having enquir'd of an old experienced Tradesman, of whom I bought an excellent Mortar of *Lignum Vitæ*, how long he had kept the Wood in seasoning before I had the Mortar, he answered me (if I much mis-remember not) 20 years, under which time it is not fully season'd for some purposes; of which opinion of his, having occasionally spoken to the lately mention'd Turner, this experienced Workman much confirmed me in it; as he likewise did in an Observation I not long since made about the slow and unperceived motion that may be, not only in the more loose and fugitive Aqueous parts of *Lignum Vitæ*, but in far more unlikely ones. For he told me that he had often found, in Turning that Wood, Cavities of several sizes in the very inward and solid part of the Wood (which every way encompass'd them) and in those Cavities considerable quantities of a certain Gum, much cryed up by some for an Anti-Venereal Medicine.

The use I would make of these Examples is this, That since so solid a Body as a Trunk of *Lignum Vitæ* is, when the Tree is newly fell'd, may require so long a time as 20 years, or upwards, to be seasoned (*i. e.*) brought to its full compactness and toughness; and since the account upon which time seasons Wood seems to be this, That by degrees the looser Aqueous, and more fugitive parts exhale into the air, whereby the remaining solid ones are brought into a closer order, and have leisure to be so placed among themselves, as is most convenient to make their Texture firm and durable: it will follow, that even in the internal parts of this solid Wood there must be, not only in the looser and lighter Corpuscles, that
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extricate themselves, and exhale away, a true local motion, though much too slow to fall immediately under the discernment of our senses. And, if the lately mentioned Gum be either totally, or so much as in part generated, as to sense, after the felling of the Tree, as some Analogous Instances that I have elsewhere taken notice of, make it probable; then the Example will further be considerable to our present purpose, by shewing that a substance so gross, and so little volatile in comparison of the Aqueous parts, as is the brittle Gum I speak of, may permeate to a great thickness, a very solid and inanimate substance; which cannot be done without an intestine, though insensible motion among the parts of the Wood, and probably a marvellously slow motion of those of the Gum.

SECT. VII.

But it will possibly seem more strange, that very thin pieces of Wood, and those saw'd off from a Tree of a much looser Texture, should be much longer in seasoning than that solid and ponderous Wood we have been speaking of. And indeed this discovery is not to be made, as in *Lignum Vitæ*, by the brittleness, or other obvious qualities in the Wood, but by a subtler way; and accordingly having purposely consulted with the Makers of Musical Instruments, and with some ancient Musicians, I was much confirmed by them in my opinion: And I remember, the last Maker of Viols, Lutes, &c. of whom I enquired of what Age he thought such Instruments, especially Lutes, ought to be to attain their full and best seasoning for sonorousness; he reply'd, that in some of them 20 years would be requisite, in others 40, according to the nature and thickness of the Wood, and other circumstances. But an Ancient Musician that was present.

present at what was said, inform'd me that there were some famous Lutes, one or two of which he nam'd to me, that attained not their full seasoning and best resonance till they were about fourscore year old. And thus much for inanimate Vegetable substances.

SECT. VIII.

As for calcin'd Stones made up into Lime, and sorts of raw stones, I have already observed from the credible relations of Masons and others, that the Walls in some Buildings attain not their hardness and solidity till they are 40 years old, or perhaps much ancients; and since in gradually proceeding to this degree of solidity, these Walls resemble the seasoning of *Lignum Vitæ* formerly explicated, the motion of the internal parts may be argued from the change of Texture as well in these as in that.

And, if I would rob other Tracts (to which they more properly belong) I could here easily adde some such Instances of the hardening and softning of Stones by time, as would much confirm what I have now been delivering; but I shall rather chuse to confine my self here to the two Examples following, not taken notice of in Quarries or by Masons.

The first is, That there are Marchasites, consisting as well of a stony as of a metalline substance, which, though harder than many other sorts of Stones, and even than Marble, have yet so great a motion in their internal parts, that if they be expos'd to the air, not only they will have a Vitriolate Efflorescence, if I may so speak, on their superficies (as I have observ'd in divers other Marchasites) but they will in Tract of time burst the Stone in Pieces; of which sort I had sometime since, and I hope I have yet a bulky Marchasite that I procur'd
from

from a Virtuoso that lives just by a Vitriol-work, whether these among other Vitriol-Stones are brought, and where this Stone being chosen for its largeness, was taken up and carefully kept by that ingenious person till it burst of it self, and till I sent for it. And to satisfy myself a little further, that the internal parts of Marchasites may be as well dispos'd to be vitriolated as the external; I remember I broke a hard Marchasite that I had from another place, and laying it some short time in a Chamber-Window, I found the new superficies made by the Tracture about the middle of the Stone to have acquired an Efflorescence of a vitriolate Nature.

The other instance, which is very odd, and much talk'd of, is this: An ingenious Gentleman of my acquaintance, casually meeting me one day, told me that he had a Turquois-stone, which if he were not mistaken had a wonderful property, for there being in it several spots of Colours differing from the rest of the Gem, these spots seem'd, though very slowly, to move from one part of the stone to the other. And this he thought himself to have taken notice of for very many Months (perhaps a couple of years). This Relation seem'd so strange that the Relator was not at all surpriz'd, when to ascertain my self of the truth of it, I desired to have the Ring this stone was set in, for a while in my own keeping, to which he readily assenting; besides that I took very heedful notice of the scituation of the spots, I employ'd a very ingenious youth that then lived with me, and was skill'd in drawing, to make the Picture of the stone with the spots as they were then placed, and afterwards to have a watchful eye upon it, and from time to time (as once perhaps in two or three weeks) to draw a new Picture of them; by comparing several of which Pictures, it was unanimously concluded that the spots did shift places in the Turquois, as if the matter they consisted of
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made its way through the substance of the stone: As we lately noted that the Gum of *Lignum Vitæ* seem'd to do through the substance of the Wood: And as far as we observ'd, the motion of these spots was exceeding slow and irregular, though perhaps it might have been reduc'd to a somewhat less uncertainty, were it not that by an unwelcome accident we were deprived of the opportunity of continuing our Observations long enough. And this brings into my mind, that the Turquois being a stone, of which I had met strange stories in good Authors, I once asked several questions about it, of a noted Jeweller; and enquiring among other particulars, whether he had not observ'd some changes that seem'd spontaneous in the substance or colour of the stone? he reply'd that in some few Turquois's he had observ'd two differing Blews in differing parts of the same stone, and that one of those Colours would by slow and unperceived degrees invade, and at length overspread that part of the stone, which the other Colour possessed before. I shall here add, that the same Gentleman that lent me the spotted Turquois, shew'd me afterward an Agate Haft of a Knife, where was a certain Cloud, which he told me an ingenious Person had for some years observ'd to remove to and fro in the stone, and had a while since to convince the Relator lent him the Agate, of whose Phænomena he promis'd me an account, when he shall have had the stone in his custody for a competent time, till the expiration of which, it may suffice to have said of this Agate what I have now related.

SECT. IX.

But because that Diamonds and Glafs are generally looked upon, especially by Chymists, as Bodies of the closest and firmest Texture that Nature and Art afford,
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if we could shew an intestine motion even in the parts of these; fitter Instances for our purpose could not in reason be desired: I shall venture to say something of each of them, though what I have to say about Diamonds, is propos'd rather to ground a suspicion of what may be, than a demonstration that it must be.

In the first place then, to remove that prejudice that may be entertain'd from the incomparable hardness of Diamonds (which I confess experience has made me admire) as if Bodies so hard and solid could not have their parts put into motion but by some extraordinary, not to say, prodigious force; I shall only repeat here what I have elsewhere shewn, that Diamonds are Bodies that easily enough become actually Electrical, and that some Diamonds (of which sort I have a small one by me) will by rubbing upon a cloath be brought to shine in the dark, the Quist of both which transient Qualities requiring a change of Texture even in the internal parts; and the Friction that produces that change, doing it immediately by putting the parts of the stone into local-motion, it may be thence argued, that a very moderate force may suffice to beget an internal motion in the inward Particles of Diamonds themselves.

And I am not sure but that more hidden Agents may make impressions upon these hardest Bodies. For in a Ring that I am wont to wear on my little Finger, which has no Diamond, save one more than that shining one I lately mention'd, I have I know not how often seem'd to my self to observe a manifestly greater clearness and sparklingness at some times than at others, though I could not refer it to the clearness or dulness of the weather, the moisture or driness of the air, the superficial clearness or foulness of the stone, or any other manifest

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cause I could think of. And in this I was the more inclin'd to think I might not be mistaken, because, besides that the notice I took of it, was frequent, I have by me a rough Diamond just as it came from the Rock, in whose Electrical faculty I have taken notice of changes as to the degree of strength wherewith it attracted, and that within the compass of a very little time, though I could not find any cause whereto I could refer so surprizing a Phænomenon. And I must not here omit, that chancing one day to shew the newly mention'd Diamond Ring to a very ingenious Lady that used to wear in Rings and Bracelets, store of those Gems, and telling her what changes I had taken notice of in the Diamonds; she who had observ'd more about Gems than any Lady I had yet met with, appeared but little surprized at what I told her, and affirm'd to me that she had divers times observ'd the like alterations in some Diamonds of hers, which sometimes would look more sparkingly than they were wont, and sometimes far more dull than ordinary. And when I objected, that possibly that dulness might be imputed to the weather, or some casual foulness of the surface of the stone, she reply'd that she had been aware of those circumstances, rubbing the stones clean, and otherwise taking care to secure an Observation, which she had made too often to have deceiv'd her self in it. If I remember aright, I have elsewhere mention'd how I saw a considerable, but Cloudy, *Hungarian* Diamond, which the Owner would have presented me, made clearer by lying for some time in a cold Liquor, wherein he affirmed that upon his keeping it longer the stone would lose more and more of its cloudiness; and what I my self saw suffic'd me to argue, that changes may be produc'd even in the inward parts of such Diamonds by Agents that act without any appearing Violence.

SECT. X.

And if it be true that Diamonds, as I elsewhere observe about many other stones, may be generated from time to time in the bowels of the Earth, it may not perhaps be absurd to imagine, that even true Diamonds, that seem perfect, and are fit for Rings, may long continue to have an insensible motion through the whole stone, whereby the Corpuscles it consists of are dispos'd into a more convenient Texture for the constituting of an extreamly hard body. For though it be taken for granted, that Adamantine Bodies, because they are generally exceeding hard, are equally so, yet that supposition is not favoured by Experience. And I remember, that to satisfy my self further about such matters, I repair'd to an ancient Artificer eminent in his Trade, which was the cutting and setting of Diamonds, and that having demanded of him whether he found that all Diamonds were of equal hardness; he answered me, that having dealt in these Gems near 20 years in *Amsterdam*, and divers years in *England*, he perceived that there are of later years brought over worse and worse sorts of Diamonds, and that he finds several of the recent Diamonds so soft and brittle in comparison of those he was anciently wont to set (and which he with other Jewellers called Diamonds of the old Rock) that he is often afraid, and unwilling to meddle with them, when they are brought to him, lest he should spoil them in the cutting, or polishing. But this I only repeat historically, till further observation shall discover whether these are Diamonds not yet fully ripe, and capable of growing harder by further maturation, or whether they be of a peculiar sort of Diamonds whose nature it is to be always softer than those of the old Rock.

SECT. XI.

This brings into my mind a confirmation of the unequal hardness of Diamonds, whatever be the cause of it, which I met with in a little Book lately Published in his own Language by a Frenchman, who giving his Reader an Account of the Eastern Diamond-Mines from the Relation, as he affirms, of a late Eye-witness, speaks thus of the third and last Mine called *Gazerpoli*, *They are very clear, and of a good Water, but they cannot be ground by mutual Attrition (if I understand the Term he uses) but with stones of the same Mine;*

Egrezes, pag. m. 17.18.

for if one should employ for that purpose the stones of another Mine, those of Gazerpoli would be broken in pieces: They do also easily break upon the Wheel, and those that are not vers'd in the knowledge of stones may easily be deceived (in them.) Of which our Author addes the Example of a Portuguais, who refusing 1200 Crowns for one of them at Ligorn, when he went to have it cut at Venice, it broke upon the Wheel into fifteen or twenty pieces.

Another Example that seems to make more for our present purpose is afforded by the same Author, speaking of the second Mine, which breeds the greatest stones called *Gane* or *Colonor*; for he says, that *sur la plus part, i. e.* upon the most of these stones after they are cut, there appears always as it were a kind of greasiness or unctuousity, which invites you ever and anon to have recourse to your Handkerchief to wipe it off, which one would guess to proceed from some insensible Effluvium, that exhaling out of the stone comes to be check'd and condens'd by the air on the superficies of it, as it happens to sweat on the skins of Animals: the truth of which conjecture I would examine by very nice scales, if I could procure such Diamonds.

SECT.

SECT. XII.

And because Rubies, though inferior in hardness to Diamonds, are yet harder than most other Gems, and much more than Marble and the like courser stones, I will not omit on this occasion, what was more than once affirmed to me by an observing Lady, whom, if she were not too nearly related to me, I could scarce mention without an Elogy. For casually casting my eye upon a fair Ruby she wore upon her finger, and desiring to consider it more attentively, she pull'd off the rich Ring 'twas set in, and reaching it me, told me 'twas worth my curiosity to consider it. For besides that 'twas so fine a stone, that 'twas thought worth being left her as a Legacy by a great Lady (her dear friend) that was famous, as I knew, for the variety of the rich Jewels she was Mistress of; this Ruby would not unfrequently vary the degrees of its lustre she knew not why. For sometimes it seem'd to be ennobled by a more vivid fire than ordinary, and at other times it would be manifestly more dull and cloudy than 'tis wont to be: and this not imputable, as she expressly assur'd me upon repeated Observations, to the Cloudiness of the Weather, or any superficial foulness of the stone. And that I might be convinc'd as well as she her self was, she desir'd me to rub it very clean, and then take notice of the present lustre of it, of which e're long she presum'd she could shew me a manifest alteration (for I was then come to visit her and pass some weeks with her in her house) but my occasions calling me away within a few days after, I had not time to wait for the event of her promise.

How far what has been said concerning Diamonds may be allowed to be Argumentative towards the scope of this Discourse, I shall willingly leave to the discovery
of

of time, and further Observation; the mention I have made of the foregoing particulars, having been invited partly by the nobleness of the subject, which made me willing to adde these Relations to what I have elsewhere written about them; and partly because thus much at least seems probably deducible from what I noted about the exciting of Diamonds by rubbing, both to attract, and to shine, that notwithstanding their incomparable hardness, an intestine motion of their minute parts may be without any considerable violence quickly produc'd.

S E C T. XIII.

And now 'twill be time to consider the other Body I promis'd to take notice of, namely Glass. For this being thought so compact and firm a Body, that 'tis indistructible by Art or Nature, and being also of so close a Texture, that the subtlest Chymical Spirits (that are yet known) cannot pervade it, and lastly having given such proofs of the fixedness of its parts, as to have long endur'd the violence even of a Glass-house-fire, we can scarce imagine a Body more unlikely to have any motion amongst its component Particles: and yet that they may not be always at perfect Rest among themselves, I have been induced to think by the following, and the like Observations.

First, having enquired of a famous and experienced Maker of Telescopes, as well as of those that use such Instruments, whether he did not observe that the Venice-Glasses he employed would sometimes crack of themselves whilst they were yet in Plates; and sometimes do the like after they were ground into Convexes, and polished up; he answered affirmatively. And though it seem'd improbable that Glasses brought so
far

far off as from *Venice*, and many of them kept a good while here in *England* before there be occasion to grind them: and perhaps longer after their having been ground before they crack, should after all this time retain an internal motion among their component Particles: yet I have been induc'd to conjecture that some saline Corpuscles more numerous than the Nature of the Glass requir'd, may, by degrees though slow and unperceiv'd, so tend towards the superficies of the Glass, as either to get out of the Pores of it, or crack, or burst the Glass in endeavouring to force their passage outward. For having purposely enquired of the above-mention'd Artificer, and some other observing men that deal in Optical Glasses, whether it had not been taken notice of, That there would sometimes be, especially in Winter and very moist Weather, a kind of Efflorescence of a saltish taste manifestly discernable upon the surfaces of their Glasses; I was answered in the Affirmative, especially by the above-mention'd Artificer, who having more occasion and opportunity to take notice of such things, told me that he had by tasting found these Exsudations sensibly saltish.

SECT. XIV.

And I was the more apt to entertain the lately propos'd Conjecture, because of a thick Glass Cup that I have yet by me, in the making of which, to render it the more Diaphanous, I suppos'd an over great proportion of Salt had been employed. For this Cup though for a while it continued clear and entire, yet before the ensuing Winter was ended, though it did not so crack as to fall to pieces, but still retains its former shape, yet it was flaw'd with such a multitude of little cracks,

cracks, that at a distance it looks like a White, not like a Christalline Cup.

SECT. XV.

I remember also that I have sometimes, though not often, had Vessels and other Bodies of Glass of a considerable thickness, which have of their own accord broken suddenly asunder, with noise enough to make me take notice of it. And that this did not always happen for want of the Glass's being gradually or slowly cool'd, or, in the Workmens Language, neal'd, I was perswaded not only by the spontaneous cracking, not without a loud noise of a thick and empty Glass Vessel, that had for I know not how many Months been kept locked up in my Study; but by the like Accidents, which I after found had happened unto others. For enquiring of some that made great store of Glass Vessels, as well as of others that sold them; I learned from both, that they had sometimes by their losses been made to take notice that Glasses that had been long made, and kept unemployed would break of themselves, when there was no visible outward Agent near enough to be suspected of the having broken them. And since this very Page began to be written, I had a fair Christal Vial, not too well stopt, which crackt at the thick bottom, in a Glass-Cabinet (that was fixed to a Wall) where I kept that with other choice Vials under Lock and Key; no other of the included Glasses (full nor empty) nor yet of the external Glasses appearing any way crackt or injured. Nay even great and strong Looking-Glasses are not quite exempted from these accidents. For I remember that having purposely enquired of an honest man that furnished the greatest part of *London* with large Looking-Glasses,

Glasses, whether he did not sometimes find them crack, and that with noise; he show'd me divers large Plates of excellent Glass, and assured me, that sometimes after they had been a good while in his Shop, some of them would of themselves, not only crack with a loud noise, but now and then also (though rarely) fly asunder with that violence as to break some of the neighbouring Plates though thick and strong.

SECT. XVI.

And having also a mind to enquire further, whether this disposition to break in some sorts of Glass, might not continue much longer than I had opportunity to observe, I address'd my self to an ingenious Master of a Glass-house, and demanded of him how long he had taken notice of Glass to continue sound and whole, and yet afterwards to break of it self. He replyed that he had once a great parcel of Glasses packed up, which not having the occasion he expected to vend and make use of, lay by him for a great while; and yet when afterwards he had unpack'd them, and rang'd them, in a short time a great many of them, amounting to about a fourth or third part of the whole number, cracked of themselves; and when I asked how long the Parcels had lain by before they were opened, he replyed, that 'twas as he remembred between four and five years.

SECT. XVII.

These Instances (to which I could adde divers others) I have therefore mention'd because either of the two Hypotheses in congruity whereunto they seem likeliest to be intelligibly explicable, will favour the Doctrine hitherto patronized. For according to the Atomical Theory, it may be conceived that there is a constant intestine Motion of the small parts of the Glafs upon the score of their constituent Atoms, which endeavour or tend to extricate themselves and get away, which at last they do, by breaking the Glafs in some brittle, or other fit place; where (after a multitude of encounters and evolutions) a competent number of them may happen to be got together, and find their Motion (outwards) withstood: whence may ensue so unequal an agitation there, of the formerly coherent parts of the Glafs, as to make the more agitated ones part from them that are less so; and consequently crack the Glafs. To which agrees what I have often observ'd in Chymical and Mechanical Tryals made with Glafs-Vessels, That if there be any grain of Sand or Gravel, or any little Lump of the Alkalizate matter Glafs is made of, conspicuously inclos'd in the substance of the Vessel, 'twill both be much the more apt to break, and if it do, will almost always begin to crack at that place, (whence usually as from a Center several cracks go several ways) the part of the Glafs where the blemish is, being commonly of a differing Texture from the rest (as is often manifest to the very eye) and being by that incongruous Texture disposed to be put into a motion differing

differing from, and perhaps very disproportionate to that of the neighbouring Parts.

SECT. XVIII.

I must not here stay to examine, whether or no this motion of the internal parts may not (in divers cases be made more efficacious by the penetration of some subtle and moist matter into the Glasses Pores, (especially the more superficial ones of some Glass of a looser sort) and so by degrees vitiate the Texture of the Body, and promoting the Agitation and swelling of the saline Corpuscles, enable them to burst the Glass, after some such manner as the Marchasites I lately mentioned, came to have a vitriolate Efflorescence, and even to be burst by the operation of the Air; this, I say, I must not now stay to examine, because I would hasten to propose the second Hypothesis, and tell you that (else) we may, congruously to what we elsewhere discourse, imagine, That in tract of time, there is produced in some parts of a Glass a Texture that makes it resist more than it did formerly the free passage of the Æther, or some other subtle matter; that was accustomed (perhaps stream-like) to permeate it before; which transient matter now finding its passage obstructed (and perchance almost quite hindered) by the vitiating of the Pores of the Glass, or some other (inconvenient) change of Texture in it, and endeavouring to continue its wonted motion through it, does so stretch the Pores, or otherwise offer violence to the Texture of the Body, that it causes a divulsion in

the parts, which according as it is more or less forcibly or suddenly made, does either barely crack the Glass, or make it flye asunder. To the precedent Doctrine these two things agree not ill: The *First*, That Glass is a Body easily made Electrical by rubbing, which makes it probable that its Particles may easily be put into motion. And the *Second*, That such a Divulsion may be made in Glass by but an unequal motion between the neighbouring parts; as may appear by the Chymical practice of cracking Glasses, which they often think fit to do, only by applying a red hot Iron to the place till it be sufficiently heated, and whilst it is very hot, moistening it with cold water (or even Spittle) which by cooling the part that it touched, and consequently checking the Agitation of the Corpuscles it meets there, whilst the contiguous parts retain their former vehement Agitation, occasions a discontinuity or divulsion in the Glass, some of whose parts are in so swift, and others are in so slow a motion.

SECT. XIX.

And on this occasion I shall adde chiefly, because I would not pretermit so considerable a Phenomenon, That even when Glass seems to have lost the degree of heat that one would think necessary to have its shape or bigness sensibly alter'd, there may remain yet so much agitation in the minute parts, as, when they are modify'd by the Air, by the Cold, or by some other invisible Agent, to make them alter the bulk or shape of the whole Vessel they compose; and that (which one would not expect) by enlarging

larging the Vessel, at least if we allow not in the case any change of Figure: For it has often been observed in those Glass-houses where they work White Glasses, as they call those that are pure and clear, that when they have blown Glasses into a mold, to give them more exactly the desired shape and size, these Glasses when they are cold cannot well be put again into the same mold they were blown in, and require that the Cells of Garde Vials that are to receive them be made a little larger: which Observation an eminent Artificer of my acquaintance that gets considerably by fitting fine Glasses to Cases; has much confirm'd to me by his complaints of the inconvenience, easie to be incurr'd, by the not knowing, or not remembering so unlikely an Effect of the cooling of Glass. But I must not prosecute the consideration of these, and the like Phænomena, nor examine which of the preceding Explications is preferrable, having mention'd them, as I was saying, chiefly to shew, that which of them soever we pitch upon, it will argue an intestine motion in the Corpuscles of the Glass, which motion we shall think may be very slow, if we consider how long a time it is on some Occasions in producing its effects before it brings them to be discernable to our senses.

SECT. XX.

Having thus made it probable, that among the parts of such solid Bodies, as I have hitherto instanced in, there may not be such a perfect Rest, as is generally believed; it will I suppose be expected that I should now draw this consequence from what has been said, That there is no such thing as absolute
Rest

Rest in Nature. But since at my first mentioning this Paradox to you, I proposed it but as Problematical, and since I consider that we are not yet sure, but that though many of the parts of solid Bodies may not be *always* moveless, yet some others of them may *sometimes* for a while at least be at perfect Rest: I shall conclude as I began, and without resolutely denying that there can be any such thing *in rerum naturâ*, as absolute Rest, I shall content myself to say, That 'tis not either absurd to doubt whether there be or no; nor improbable to think that there is not, since we have not found it in those very Bodies, where with the greatest likelihood it might have been expected.

F I N I S.

